

ogr
01
n

H^o Biogr.

101 m

Baily

<36634554900014

S

<36634554900014

Bayer. Staatsbibliothek

H^o Biogr.

101ⁿ

SUPPLEMENT

ACCOUNT

RECEIVED BY FRANCIS BAILY, Esq. F.R.S.

RECEIVED BY FRANCIS BAILY, Esq. F.R.S.

LONDON, (January) 1853

Presented by the Author to

The Royal Academy at Munich

SUPPLEMENT

TO THE

ACCOUNT

OF THE

REV^D JOHN FLAMSTEED,

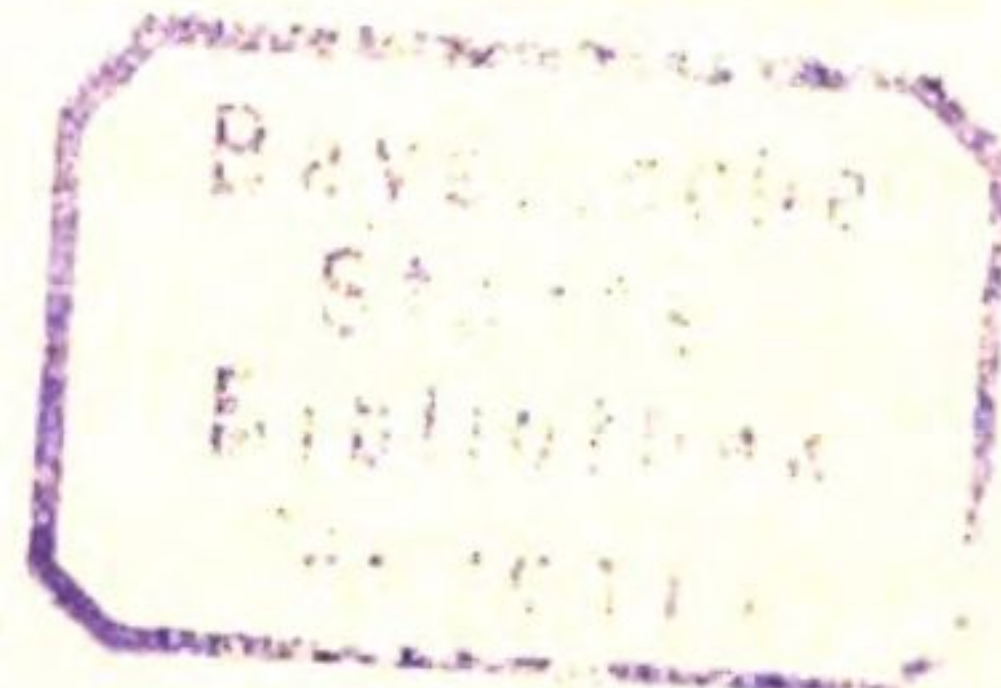
THE FIRST ASTRONOMER-ROYAL.

By FRANCIS BAILY, Esq., F.R.S.

&c. &c. &c.

LONDON. (January) 1837.

PRINTED FOR DISTRIBUTION AMONGST THOSE PERSONS AND INSTITUTIONS ONLY,
TO WHOM THE ORIGINAL WORK WAS PRESENTED.



LONDON :
Printed by WILLIAM CLOWES and Sons,
Stamford Street.

LONDON. (January) 1887.

THESE ARE THE PROPERTY OF THE
BRITISH MUSEUM LIBRARY



SUPPLEMENT.

THE following sheets are intended as a *Supplement* to my *Account of the Rev. John Flamsteed*, which was printed in the year 1835 at the public expense, for a limited distribution only; a work that has excited a degree of interest much greater than I could have anticipated, and probably arising from the prominent manner in which some of the circumstances, relative to the conduct of two eminent and distinguished individuals therein mentioned, have since been brought forward, by other parties, before the public. For, whilst on the one hand the brilliant talents of Halley and the still more transcendent mind of Newton have been set in opposition to the less splendid yet very useful labours of Flamsteed, and arguments drawn from this relative position of the respective parties, we see that on the other side the ardent zeal of the Astronomer Royal, his piety, his accuracy, his integrity, and his independent spirit, have been arrayed against the alleged misconduct of his two powerful opponents. On the accuracy or propriety of the various opinions that have been formed and expressed upon the principal features of the dispute in question, it is not my intention in this place to make any remarks: all the facts connected therewith, as far as I have been able to collect and arrange them, are before the public, and each reader will form his own conclusion according to his own particular view of the case.

But in the course of these discussions, certain topics have been brought forward on which it appears that some further explanation and information are

requisite to enable the general reader to arrive at an unbiassed and correct opinion. Could I possibly have foreseen the precise points that would most forcibly have attracted the public attention, and that would have been urged as the most powerful arguments on either side, I should have considered it my duty to have enlarged upon them more fully in my Preface to that work, for the purpose of enabling the reader to become better acquainted with all the facts and circumstances of the case; still leaving him to draw his own conclusions therefrom. I had indeed imagined that the abstract of the principal events recorded in Flamsteed's autobiography, which I drew up in the Preface above mentioned, was sufficient to prevent any misconception or misconstruction of the material facts, or any confusion of the several diversified matters that occurred in the long period of more than fifty years. In this, however, I appear to have formed a mistaken opinion; as I find that some persons, whose talents I highly respect, and from whose judgment I seldom consider it safe to differ, have come to a different conclusion on some of these points. But perhaps the confusion was one of the unavoidable evils arising from the mode adopted of publishing such a mass of documents in their *original*, rather than in a compressed and reduced state. It therefore becomes necessary to enter a little more at large upon some of the points on which such misconception or confusion seems to have taken place.

It cannot be disguised that the quarrel between Newton and Flamsteed relative to the printing of the Greenwich observations, has arrested a much greater portion of the public attention than any other incident recorded in Flamsteed's life; and indeed greater than its relative importance seems to merit: and Newton's admirers have, as might have been expected, shown a natural desire to remove from him every appearance of misconduct arising out of that dispute. In doing this however it seems to me that, in some instances at least, the tendency of their remarks has been to exculpate Newton not so much by a direct refutation of the charges adduced by Flamsteed, as, by attempting to lower the moral and scientific character of Flamsteed himself in public opinion, thus to show that Newton was most probably right in the line of conduct which he pur-

sued. This course however can scarcely be tolerated at the present day : neither is it just to the character of Flamsteed, (nor indeed to that of Newton, which stands too high in the general opinion of mankind to need such support) that the decision should rest on such grounds. The mere fact of mental superiority, which no one is disposed to deny, ought not to weigh one feather in the scale of justice : and the case must be decided solely on its own merits. As some of the arguments which I have heard adduced, appear to me to be founded on a misconception of facts, and on a confusion of circumstances and dates, I trust I may be excused from attempting to remove any cloud of this kind from Flamsteed's character and conduct : and shall therefore at once proceed to discuss the several points that have been urged in support of those arguments, and which, as far as I can classify them, may be arranged under the following general heads.

1°. That Flamsteed did not understand and therefore could not justly appreciate Newton's theory of Gravitation, and more especially his new theory of the moon : consequently that he was not fully aware of the great assistance that could be afforded, by his observations, in the formation and verification of that theory.

2°. That Flamsteed showed an unwillingness, and even an objection, to furnish Newton with the requisite lunar observations to enable him to perfect that theory ; a reluctance which consequently endangered the completion of that important work.

3°. That similar feelings induced Flamsteed to raise frivolous and vexatious objections to the printing of his astronomical observations, when it was afterwards undertaken by the Government : and that the delay was occasioned solely by his own misconduct.

4°. That in order to obtain the Catalogue of the fixed stars, for publication, which was sealed up by mutual consent, the Referees had a *right* to break the seal of the packet in which it was contained.

Now, these assertions have been so frequently and so strongly adduced (either wholly or in part) in palliation of Newton's conduct towards Flamsteed, in the disputes which arose between them in the latter period of their lives, respecting

the publication of the Greenwich observations, as already related in the preceding part of this history, that I fear I have not, in my Preface, been sufficiently circumstantial and clear in my attempt to connect the chain of varied incidents that befell Flamsteed during his long and chequered career. Indeed, such charges were so totally unexpected by me, and in my opinion so much at variance with the recorded facts, that I should have considered it as going out of the way, to have anticipated the necessity of any defence for Flamsteed's conduct. With respect to the first two charges, even supposing them to be true to the fullest extent (which however I by no means admit), they can have no connection whatever with the subject matter of these disputes: and the question of right or wrong must be decided on totally different grounds from any that are involved in those assumptions. For, Newton had left the subject of the lunar theory long before the quarrel which arose respecting the manner in which the Greenwich observations should be printed. The dispute on that subject is sufficiently painful, and its details in some points doubtful and unsatisfactory (since we have frequently only one side of the case to decide us in our inquiry), without our rendering it more distressing and intricate by mixing up with it matters that are wholly foreign to the subject. My object therefore is to clear the way for a more full and perfect understanding of the several subjects here alluded to, by endeavouring to remove certain misconceptions which I fear may have possessed the public mind, arising probably from a confusion of dates and circumstances, or from an inattention to the precise state of the science at the period under review. And in doing this, I trust I shall not be considered as attempting (as it certainly is far from my wish) to cast any improper reflection on the character and conduct of others: my sole object being to place those of Flamsteed in their true light.

As the *first* of the above-mentioned assumptions will lead me into the longest inquiry, I must entreat the patience and attention of the reader whilst I bring forward, for the sake of elucidating the subject matter of such inquiry, a few circumstances to show the state of the lunar theory immediately preceding the

publication of the *first* edition of the *Principia*, and the principal difficulties that were to be encountered in that research. It has indeed been justly remarked by Mr. Whewell, in his pamphlet entitled *Newton and Flamsteed*, when speaking of the discovery of the theory of Gravitation, that “we of the present day are accustomed to consider this immense step as effected at once, on the first edition of the *Principia* in 1687; but, we may easily convince ourselves that this was not so: even under the most favorable circumstances a vast theory like this could not make its way at once.” The fact is that, with respect to the lunar theory, which more immediately concerns the subject of the present remarks, although its general principles were indicated by Newton in the first edition of the *Principia*, yet it was not till the publication of the second edition in 1713 that he more fully (but still incompletely) developed those principles relative to the lunar motions which have led to such important and valuable results in the hands of his *successors*: for, it lay dormant nearly half a century before it was rendered essentially serviceable to the purposes of astronomy and navigation. If, therefore, even after the appearance of this latter edition, we assent (as I think we fairly may) to Mr. Whewell’s opinion that “no man of Newton’s standing thoroughly accepted his views,” it may very reasonably be inferred that, *prior* to that time (which is the period now under review), his principles, more especially as they relate to the moon, were not fully understood, and consequently not generally received. And it is obvious that, in this early stage of the inquiry, the most effectual mode of stamping their truth and their value in the public mind, would be to show the accordance of the theory with observations.

The motions of the moon are so apparently irregular, and subject to so many seeming inequalities, that (setting aside those minute ones which have since yielded to modern instruments and a more refined analysis) it is no wonder that so long a period should have elapsed before the more prominent irregularities (which were the first to be discovered) could be accurately accounted for, on any of the various systems that had, from time to time, possessed the public mind. Prior to the time of Ptolemy only one inequality of the moon’s motion in longi-

tude was known ; which was certainly a very large one, and could not long have escaped observation : by the moderns, the correction for this inequality is called the *Equation of the centre*. Ptolemy added a second, which is called the *Evection* : and Tycho Brahé discovered two others, which are respectively called the *Variation* * and the *Annual equation*. Prior to the time of Newton these four were the only inequalities in the moon's motion, in her orbit, that were known ; being in fact all that could then be discovered from observation : and although anomalies were sometimes found in the comparisons of the observed and equated places of the moon (as well there might be, from the rough manner in which the observations were made, and the imperfect theories on which the tables were formed), yet no effectual attempt to clear up these difficulties was made till the latter end of the year 1638, when Jeremiah Horrox suggested the first important improvement in the lunar theory, by considering the moon to move in an ellipse, with a variable eccentricity, and having a libratory motion of the apsides. Horrox was a young man of great talent and promise : he was only 19 years of age when he announced his new theory, and died at the premature age of 22 †. His improvement consisted principally of a new method of explaining and developing the Evection, founded on the consideration just alluded to. Ptolemy had enuntiated the problem by means of an epicycle upon an eccentric ; whilst Copernicus adopted an epicycle upon an epicycle, for the solution of the same. But, Horrox resorted to a different and more correct mode of solution ; which, as we shall presently find, was also followed by Newton. Horrox's theory is very imperfectly explained by himself : he alludes to his first attempt to improve the lunar tables, in a letter which he wrote to his friend Crabtree on September 29, 1638 ; and afterwards to a more improved method, in another letter to him on December 20th following ; in which indeed is contained the germ of his new theory.

* From a recent publication, entitled *Nouvelles recherches pour servir à l'histoire de l'Astronomie chez les Arabes*, by M. Sédillot, it would appear that the Variation was known to the Arabs in the tenth century.

† “Tenui fortunâ, librorum inopiâ, defectu consiliorum oppressus, cum aliis studiorum incommodis confligebat, duce pariter et comite studiorum destitutus, et literatorum consortio, nihilque ferè ad hæc studia comportavit præter animum suo desiderio flagrantem, et laborum omnium patientem.” *Horrocci Opera Posthuma*.

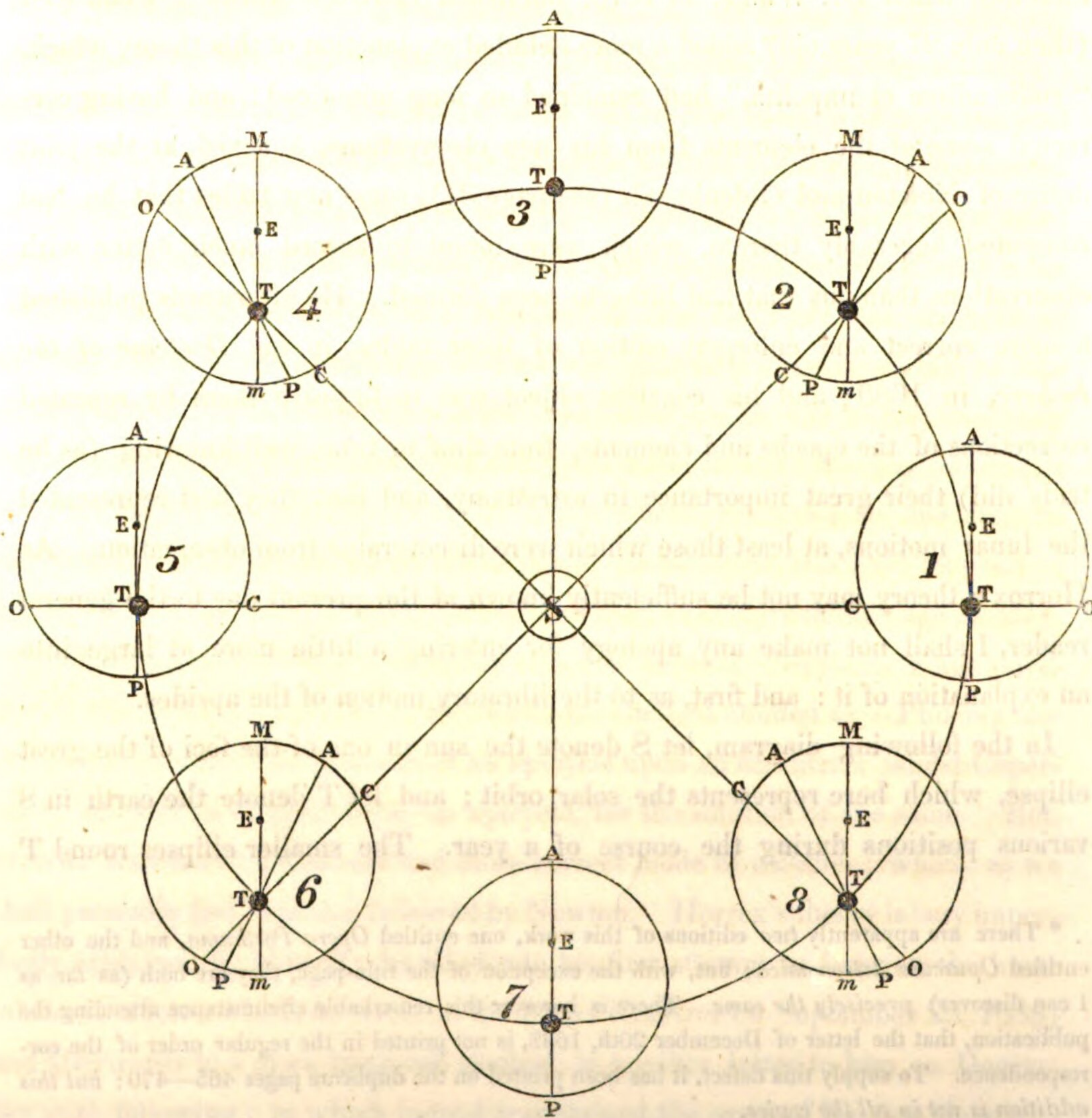
Flamsteed very soon saw the importance and advantage of this new theory, and how much the lunar tables might be improved and simplified by its adoption: therefore when Dr. Wallis, in 1673, published Horrox's works*, Flamsteed (then only 27 years old) added a more detailed explanation of this theory which, "rudis adhuc et impolita," had remained so long unnoticed: and having corrected some of the elements from his own observations, annexed, at the joint desire of Newton and Oldenburgh (see page 31), some new tables that he had computed agreeably thereto, which were found to accord much better with observations than any that had hitherto been formed. He afterwards published a more correct and enlarged edition of those tables, in his *Doctrine of the Sphere*, in 1680; and his constant object was to improve them by repeated corrections of the epochs and elements, from time to time, well knowing (as he truly did) their great importance in astronomy, and that they best represented the lunar motions, at least those which were discoverable from observation. As Horrox's theory may not be sufficiently known at the present day to the general reader, I shall not make any apology for entering a little more at large into an explanation of it: and first, as to the libratory motion of the apsides.

In the following diagram, let S denote the sun in one of the foci of the great ellipse, which here represents the solar orbit; and let T denote the earth in 8 various positions during the course of a year. The smaller ellipses round T

* There are apparently two editions of this work, one entitled *Opera Posthuma*, and the other entitled *Opuscula Astronomica*: but, with the exception of the title-page, they are both (as far as I can discover) *precisely the same*. There is however this remarkable circumstance attending the publication, that the letter of December 20th, 1638, is not printed in the regular order of the correspondence. To supply this defect, it has been printed on the duplicate pages 465—470: *but this addition is not in all the copies*.

Since this note was written I have had an opportunity of inspecting the valuable collection of MS. letters in the possession of the Earl of Macclesfield, and I find, from the tenor of some letters from Flamsteed to Collins, and from Wallis to Collins, that the letter of December 20, 1638, was omitted at Flamsteed's suggestion; he being of opinion that the numbers given in Crabtree's letter to Gascoigne, dated July 21, 1642, suited better with the heavens than those given in the letter of December 20, 1638. But Wallis considered that this last-mentioned letter gave a better explanation of Horrox's theory and method (which it certainly does), and therefore supplied the omission by causing it to be printed on the duplicate pages above alluded to. The year however is erroneously printed 1628.

denote the orbit of the moon ; in each of which let A P denote the true place of the lunar apsides, and M m their mean place : and let E T represent the eccen-

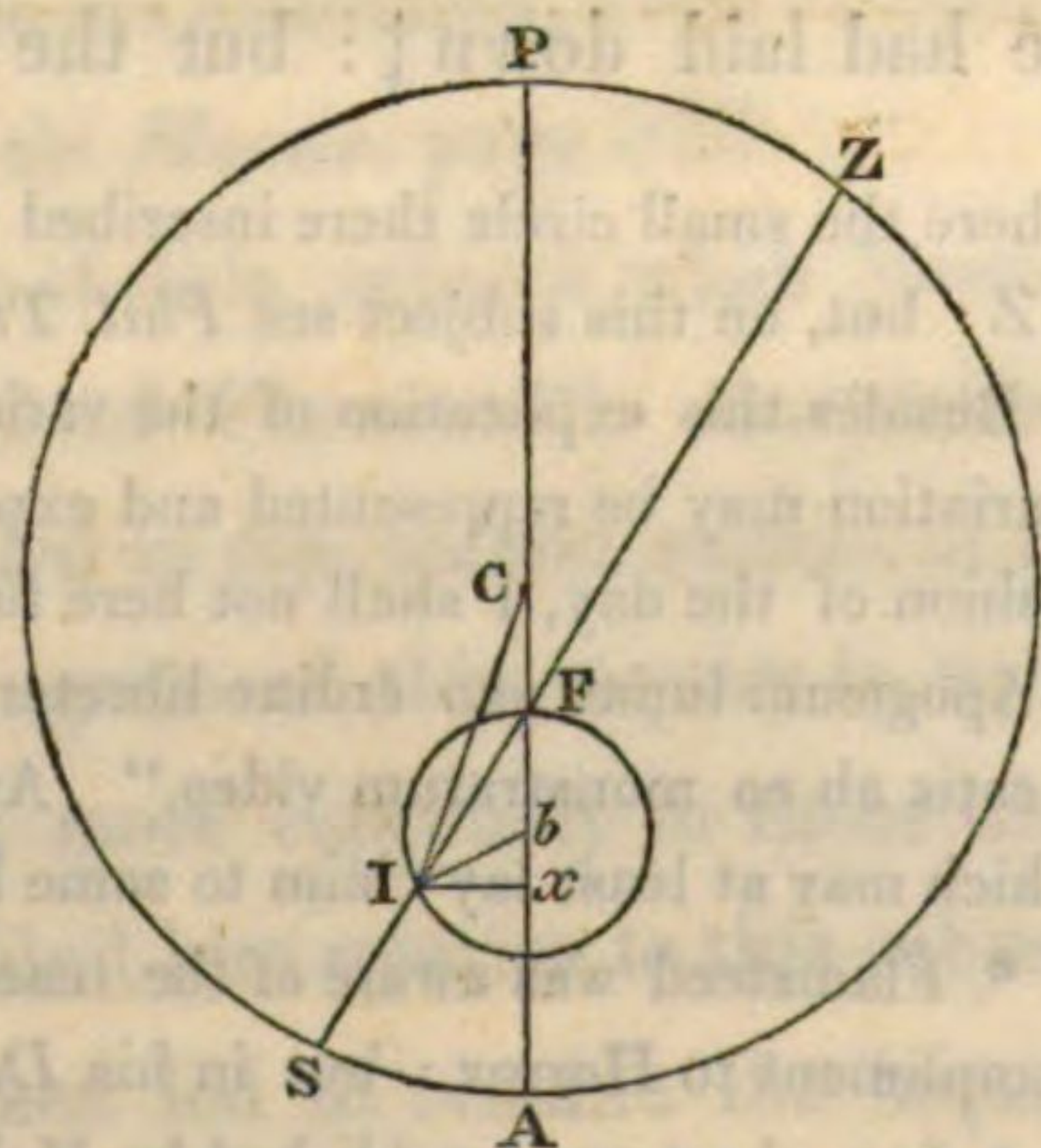


tricity of the moon's orbit. In the positions 3 and 7 the line of the apsides passes through the sun, and corresponds with the line of the sizygies ; and in the positions 1 and 5 it is perpendicular to this latter line : or, in each of these positions the true and the mean places of the apsides correspond. But in every other position there will be a discordance : the line of the apsides being drawn aside, as it were, by the action of the sun, making the variable angle ATM,

which Horrox calls the Equation of the lunar apogee: which equation is additive to the mean place of the apogee whilst the earth is going from 3 to 5 and from 7 to 1; but subtractive in the other two quadrants. Horrox then gives the rule by which the amount of this equation is to be computed*.

With respect to the variation of the eccentricity of the lunar orbit, we do not find any specific allusion to it in the letter of Horrox to Crabtree, of December 20th, above mentioned: but in Crabtree's letter to Gascoigne, of July 21, 1642 (which was written for the express purpose of explaining the new theory), he states thus much respecting it: that Horrox assumed the least eccentricity equal to 4362, the mean at 5524, and the greatest at 6686, the radius being 100000; and a rule is given for finding the correct eccentricity in all cases; but no demonstration of the rule is annexed. Flamsteed however, in his *Epilogue* to the tables (page 491), has given the following geometrical construction, in lieu of such demonstration.

Let $ASPZ$ be the orbit of the moon, C the centre, A the apogee, and P the perigee; and let Cb be the mean eccentricity. Upon b as a centre, describe a small circle whose semidiameter bF shall be equal to half the difference of the greatest and least eccentricity. Through the point F , of least eccentricity, draw the synodical line SIZ , cutting the small circle in I ; from which point draw the line Ib , and also the line Ix perpendicular to the line AP . Then will Ibx be equal to twice bFI , the distance of the apogee from the sun; and Cx will be the true eccentricity†.



* The reader must consider this as a mere outline of Horrox's theory, which is inserted in this concise form, in order that it should not interrupt the narrative. The whole of this letter, with the explanations of Crabtree and Flamsteed, are given at the end of Horrox's works, as above mentioned; where the reader may pursue the subject further, if he should be so disposed.

† We shall find in the sequel that Newton adopted a similar explication of the rule: considering however CI , and not Cx , as representing the variable eccentricity. This slight correction of the eccentricity was imparted to Newton as a secret by Halley: see page 137. See also page 134,

Agreeably to this new theory of the libratory motion of the apsides, and the variation of the eccentricity, Horrox proposed to unite the two principal equations of the lunar irregularities; namely, the Equation of the centre and the Evection: and, as the Annual equation was got rid of, by a supposed equivalent alteration in the Equation of time*, the tables for finding the corrections of the moon's place in her orbit were literally reduced to two only†. It was thus that the first lunar tables of Flamsteed were formed, which are to be found at the end of Wallis's edition of Horrox's works above mentioned; and which were afterwards improved by him in his *Doctrine of the Sphere*.

Such then was the state of the lunar theory at the time of the appearance of the first edition of the *Principia* in 1687: which, as I have before stated, contained only the germ of that mighty system, which has since been so fully developed, and which has led to such important results. Newton indeed showed that the nature of all the then known inequalities in the motions of the moon, and in some cases their quantities, might be deduced from the principles which he had laid down‡: but the enunciation of the precise law of most of these

where the small circle there inscribed (although correctly copied from the MS.) is drawn on the line S Z: but, on this subject see *Phil. Trans.* for 1675, No. 116, page 368, &c.

Besides this explication of the variation of the eccentricity, Flamsteed has also shown how the Variation may be represented and expressed: but, as this is done by means of an epicycle, after the fashion of the day, I shall not here further allude to it. In the *Appendix* to the Epilogue he says "Apogæum lunæ, quo ordine libriter, docuit Horroxius, et quidem optimè: at cur ita libriter non satis ab eo monstratum video." And he then proceeds to explain his own view of the subject, which may at least lay claim to some ingenuity.

* Flamsteed was aware of the inaccuracy of this substitution, which he at first retained out of compliment to Horrox: but in his *Doctrine of the Sphere* he restored the correct quantities.

† In a short tract published by Halley at the end of his *Catalogus Stellarum Australium*, 1679, entitled *Quædam lunaris theoriæ emendationem spectantia*, page 12, he ventures an opinion, that upon Horrox's theory it would be practicable to expound also the law of the Variation, by supposing the moon's orbit, in the line of its sizygies, to be compressed $\frac{1}{90}$ th part towards the earth, and thus reduce these equations to one only: but he says that he leaves it to others to follow out the plan. The object in those days, was to abridge the number of equations: the moderns pursue a very different mode of obtaining accuracy, and in the most recent lunar tables that have been published, the equations of longitude only (including however the Reduction to the ecliptic), amount to nearly 50: and, if every equation, arising from the developement of the several series as far as they have been hitherto carried, were preserved, they would be more than thrice that number.

‡ See especially the 11th Section of the first Book; also the 22nd, 25th, and other subsequent Propositions of the third Book of the *Principia*.

inequalities was deferred to a more favourable opportunity, when he might be furnished with better astronomical observations. In fact, until such a development of the theory had been accomplished by some person who understood the subject as well as Newton, this part of the *Principia* was a sealed book to the *practical* astronomer. Little suspicion, I believe, was then entertained that the simple law of Universal Gravitation, for the first time so modestly set forth by Newton, would eventually be followed by such vast and important results, and have opened such a wide field for inquiry, as have since been found to be derived from it: and it is no disparagement to any of his contemporaries that they did not, in this early stage of the inquiry, fully understand and duly appreciate the vast conceptions of his powerful and enlightened mind, nor anticipate the important truths which they ultimately developed. Indeed, it may perhaps be fairly questioned whether Newton himself was fully aware of the immense benefit and advantages that were destined eventually to flow from his own researches. “Il a bien établi (says Laplace) l'existence du principe qu'il a découvert; mais le développement de ses conséquences, et de ses avantages, a été l'ouvrage des successeurs de ce grand géomètre.” *Syst. du Monde*, page 376.

We now arrive at that period of the history of this subject when Newton again took up the lunar theory; and by the aid of Flamsteed's observations made those improvements in it which are published in the second edition of the *Principia*. The only recorded trace of the progress of this inquiry is to be found in the preceding part of the present work; more especially in those interesting letters which Newton himself has left behind him relative to this subject. See pages 133—160*. He appears to have been led to resume the inquiry from his having accidentally seen in Flamsteed's possession, when on a casual visit to the Royal Observatory on September 1, 1694, a list of about 150 places of the moon, with the errors or differences between the observed and computed places, which Flamsteed had drawn up for his own use, with a view “to correct the theory of her motions;” and a copy of which he gave to Newton. See

* Laplace says, “La connaissance de la méthode qui a guidé l'homme de génie n'est pas moins utile aux progrès de la science, et même à sa propre gloire, que ses découvertes: cette méthode en est souvent la partie la plus intéressante.” *Syst. du Monde*, page 379.

page 61, and also the Note in page 57. On Newton's return to Cambridge he wrote to Flamsteed, October 7, 1694, informing him that he had been comparing this list with his own theory, and that he had satisfied himself that, by both together, the moon's theory might be reduced to a good degree of exactness: and he at the same time points out to Flamsteed certain other observations that he was desirous of obtaining, and requests to be furnished with them. See page 133. The whole of these letters, nineteen in number, are extremely interesting, and indicate the working of Newton's mind in some of the steps of this difficult and important investigation, as well as in that relating to refraction*. They also make known to us the gratifying circumstance (to which I shall again allude in the sequel) that Flamsteed freely and cordially communicated to Newton *every* observation which he requested to have, for the purpose of enabling him to pursue his inquiries into these subjects with effect.

It appears from this correspondence that Newton, in his method of expounding the two principal irregularities of the moon, adopted the Horroxian theory of the libratory motion of the apsides, and the variable eccentricity of the lunar orbit; and that his deductions are founded on this hypothesis. This will be seen at once from the table for the corrections of those quantities, communicated by Newton to Flamsteed, as given in page 155; the plan and arrangement of which is precisely the same as that given by Flamsteed in his *Doctrine of the Sphere*, some of the numbers only being altered which Newton imagined would render the results of the computations more accurate†. The last letter in the series is dated September 14, 1695, at which time it appears that Newton had not quite finished his investigations of the lunar theory: and in Flamsteed's reply thereto he freely offers to communicate such further observations as may be required. From this period to the close of the year 1698 we find no trace of any ulterior proceedings: but on the 4th of December in that year Newton again visited the Royal Observatory for the express purpose of procuring some further observations which he had requested; shortly after which, Flamsteed

* See on this subject, M. Biot's excellent review of my *Account of Flamsteed*, in the *Journal des Savans* for March and April 1836, and January 1837.

† Newton enjoins Flamsteed to keep this table to himself, till he has perfected the moon's theory, because it will want correction. See page 150.

addressed to him the letter No. 42 in the Appendix, recapitulating the several alterations and improvements in the lunar theory and tables, which Newton had communicated to him, and requesting him to impart any further alterations or additions that he may have made. The following postscript to this letter is worthy of notice, for a reason that will presently appear: "In your letter you say *these* corrections will answer all my observations within 10 minutes; Mr. Halley boasts that *those which you have given him* will represent them within 2 or 3, or nearer *." I shall not stop here to recall to the reader's recollection the extraordinary letter which, in the midst of these apparently amicable and friendly proceedings, Newton wrote to Flamsteed, and which is recorded in page 166; but shall close this short sketch of the progress of the lunar theory at that period by stating that it appears to me to have been completed (at least, as far as Newton thought proper to continue it) in the course of the following, or at farthest, of the next following year, viz., *before* the year 1700. See the letter to Newton, in page 169; also that to Mr. Lowthorp, in pages 174—176; also that to Mr. Sharp, of May 30, 1702, in page 203.

Flamsteed naturally imagined that, agreeably to Newton's express promise, he should have been the first to whom the theory, thus amended, would have been communicated. "If you can have but a little patience with me (says Newton, in his letter of November 17, 1694) till I have satisfied myself about these things, and make the theory fit to be communicated without danger of error, I do intend *you* shall be the *first* man to whom I will communicate it." See also page 72 †. Flamsteed had heard that reports were in circulation that it had already been imparted to Halley and Gregory, and that they had boasted that there was now *no need of further observations*, since the place of the moon

* Newton himself had told Flamsteed that he thought he could perfect the lunar tables within these limits: and Gregory stated the same thing in his account of Newton's lunar theory. See Gregory's Scholium, reprinted at the end of this Supplement, page 735. But these assertions should be cautiously received, as I shall more fully show in the sequel.

† That this was at one time Newton's intention is evident likewise from an expression in his letter to Flamsteed, dated July 20, 1695, wherein he says, in reference to the table of eccentricities, &c. which he had sent him, "I would not advise you to spend your time in calculating by it, till I have compassed the small equations."

could be deduced from theory. The existence of those reports is, in fact, announced to Newton himself, in the postscript to the letter, No. 42 in the Appendix, above mentioned, and a gentle hint given to him of what Flamsteed expected: but with what effect does not appear. It is notorious, however, that Newton did give his new lunar theory to Gregory (at least, certain deductions therefrom, for the improvement of the lunar tables, and which Gregory has designated by the title of Newton's *lunar theory*), for it is introduced in the very words of the author, "ipsis auctoris verbis expressam," in the Scholium in page 332 of his *Astronomiæ Elementa*, first published in the year 1702. As I shall have occasion to refer frequently to this document, I have inserted it at full length at the end of the present Supplement: see page 735.

That this document was considered, at that day, as exhibiting the *practical* solution of the great problem of the moon's motions, is I think evident from the uniform testimony of contemporary writers (to some of whom allusion will be made in the sequel); but more especially from the following introductory paragraph to this very document, which is inserted in the first volume of the *Miscellanea Curiosa*, a work first published in 1708, under the direction of Halley. It is as follows:—"This theory, which hath been so long expected by all the
 " true lovers of astronomy, was communicated from Mr. Newton to Dr. Gre-
 " gory, Astronomy Professor at Oxford, and by him published in his *Astronomiæ*
 " *Elementa*. By this theory, what by all astronomers was thought most diffi-
 " cult, and almost impossible to be done, the excellent Mr. Newton hath now
 " effected; viz. to determine the moon's place even in her quadratures, and all
 " other parts of her orbit besides the sizygies, so accurately by calculation, that
 " the difference between that and her true place in the heavens, shall scarce
 " be above two minutes in her sizygies, or above three in her quadratures; and
 " is usually so small that it may well enough be reckoned only as a defect in the
 " observation." An English translation of the document is then given; which document (afterwards amended by Newton) continued for 50 years the only source to which the *practical* astronomer could appeal for what was then considered the most correct representation and determination of the moon's motions.

Whether Newton communicated this theory to Flamsteed, at the same time, or shortly after, it was sent to Gregory, does not distinctly appear: but from the tenor of Flamsteed's occasional remarks, and more especially from his letter to Mr. Lowthorp of May 10, 1700, I apprehend that the first copy he saw of it, in its finished state, was in the work of Gregory above mentioned*. In this letter Flamsteed says "Newton said something more to move me to desire earnestly "his *lunar theories and new additional tables*, but I would not understand him: "for, having been at much more pains in calculating her's and the planets' "places for him than ever his Cap. Halley was, and having had a *promise* from "him that I should be the *first* man to whom he would impart them, and that "he would impart the observations to nobody whatever without my leave, I "looked upon his imparting what he had deduced from them to Dr. Gregory and "the Captain, as a greater breach of promise than if he had imparted the ob- "servations themselves; and so would not request that as a favor which was *my* "due, and which he could not in justice grant to any other." Some of Newton's amendments of the lunar tables had, it is true, been transmitted (although in an imperfect state, see page 150) to Flamsteed by letter, as already noticed (see the letter of April 23, 1695); and Flamsteed appears to have adopted them: but I cannot find any trace that the 4 additional equations (to which I shall presently allude, and which are in fact the most important in our present view of the question, as well as the most novel) had been imparted to him during the correspondence, except a hint that there was a slight one depending on the position of the moon's node. See pages 134, 137 and 138.

I shall here pause for a few moments to make a remark or two on the use of the indefinite term *Newton's lunar theory*; an expression which has been frequently employed in these discussions, but I fear without a due consideration of the precise meaning which should be attached to it *at the period now under review*: indeed the very use of the general term *theory* is not strictly the same

* The passage in page 72, where Flamsteed, speaking of this theory, says, "At last it came to my hands," by no means implies that it was communicated to him by Newton. The transactions of many years are crowded into the paragraph from which this passage is taken.

now as at that period ; nor, at the present day is it always employed in the same definite sense. In Flamsteed's time its meaning in astronomy rather denoted rules or formulæ for constructing diagrams and *tables* that would represent the celestial motions and observations with accuracy, than the developement of the *causes* of phenomena, which is its rigid and more modern acceptation. But, in this latter sense it could not properly be used prior to the appearance of the *Principia*, since those causes were not then known : and when now adopted it should rather be styled *physical* theory, in order to distinguish it from its former meaning, which may be more properly called empirical or *tabular* theory. Newton himself occasionally confuses the two terms, and even amongst the most modern writers they are oftentimes not sufficiently distinguished, either in astronomy or any of the other sciences. With respect however to the lunar theory, we of the present day, who see this subject in its most complete form, in a shape very different from that in which it was left by Newton, new modelled and vastly extended by a long list of highly gifted mathematicians, and its accuracy tested by the most rigid proofs, must not imagine that it was presented to Flamsteed's view in any thing like so impressive and perfect a form, or that it *then* carried the same weight of conviction, or meant the same thing as at the present day. No person *now*, whatever doubts he may harbour in his own mind respecting the Newtonian theory, will venture to express them publicly, for fear of having his judgment denounced as totally worthless : but, in the infancy of the doctrine, the case was wholly different, and laboured treatises were requisite to prepare the public mind for its adoption. In fact, we find that even at the end of fifty years after the first promulgation of this theory, during which period we might naturally suppose that it would obtain additional credence and support, no less than three of the first and most distinguished promoters of the Newtonian theory, men of the highest talents, and who by their labours have tended most to establish and confirm it (Euler, D'Alembert and Clairaut), each on his own separate view of the case, hesitated at first to accede to its truth, because the results of their laborious investigations did not fully accord with astronomical observations. If then such difficulties accompanied the subject, in the state in which Newton

finally left it, with all the last corrections and amendments drawn from the resources of his own powerful mind, how much less satisfactory and convincing must have been the comparatively meagre statement published by Gregory in the Scholium above mentioned, and which he has designated with the title of, and which indeed was what, *at that time*, was generally known, and received by the public, as the *Newtonian lunar theory*. A bare inspection of that statement will show the reader how little of what is *now* termed and understood as Newton's theory of the moon was comprehended under the title above alluded to; and that in fact this document, although evidently drawn up by a master-hand, ought rather to be called *Rules* for computing lunar tables, than a *lunar Theory*: inasmuch as it partakes more of that character, and moreover was not accompanied with any demonstration*. And as it is necessary, in order to preserve an accuracy and uniformity of meaning in this inquiry, to mark by some definite term the distinction between this first special enunciation of the lunar theory by Newton, and the more extended developement of his principles by the moderns, I shall designate the document, published in Gregory's work, by the title of Gregory's *Newtonian rules* for constructing lunar tables; restricting the higher and more acknowledged title of *Newton's lunar theory* (that is, of his *physical* theory, in contradistinction to the empirical or *tabular* theory above mentioned) to the more modern exposition of the moon's motions deduced solely from his principle of Universal Gravitation.

Thus much, by way of definition; and also (if the reader thinks it necessary) by way of apology for Flamsteed's supposed ignorance of Newton's methods†. But, I shall now proceed to show that the document above mentioned (which, as I have already stated, is inserted *verbatim et literatim* at the end of this Supplement) is, in its two principal equations at least, rather an adaptation of Horrox's theory to the then existing lunar tables, than the exposition of a new theory founded *solely* on the Newtonian system of Gravitation: and as such

* Gregory calls it "*theoriam in praxi usurpatam*."

† I am in possession of the copy of the first edition of the *Principia*, which was presented to Flamsteed by Newton: and, from the MS. notes in the margin of the book, it is evident that Flamsteed had at least *read* the work with attention.

must have been perfectly familiar to Flamsteed. Indeed, as far as regards the four principal inequalities of the moon, the alterations and amendments made by Newton were neither new nor (comparatively) very important*; being principally confined to the correction of the epochs and the constants of the arguments: the materials for those corrections being derived from Flamsteed's own observations, and the mode of applying them being the same as that which, for many years past, Flamsteed had been in the habit of adopting†. Newton united (as Horrox and Flamsteed had previously done before him) the Equation of the centre and the Evection: and enunciated the rule, for determining the amount of the correction, on principles similar to those propounded by them. If therefore it be requisite that this part of the subject should be designated by the title of a theory, it ought in strictness to be called *Newton's Horroxian theory*; the term, in fact, which Flamsteed adopts in his letter to Dr. Wallis. See page 668.

The public therefore can now judge whether it is likely that Flamsteed, who had been studying and elucidating Horrox's works for the last 30 years, and who had during the course of a long life been in the habit of making and correcting astronomical tables of all kinds, should not have understood, and fully understood, *this portion*, at least, of what has been called the new lunar theory: and whether also he must not have been fully aware of the important advantage and assistance which was to be derived (in making the corrections) from the use of accurate and numerous observations. The following remarks on this very subject are attributed by Delambre to M. Clairaut: “Quant à ce qui

* It has, I believe, been generally imagined that the annual equations, for the correction of the lunar apogee and node, were first introduced by Newton: but, it would seem from a note which Flamsteed has written in the margin of page 333 of his copy of Gregory's *Astronomiæ Elementa*, (which is preserved in the library of the London Institution,) that they were communicated to Newton “per Flamsteedii tabulas et determinaciones.” I am unable to contradict or confirm this remark, from any documents which I have seen. See, however, page 698.

† Newton showed that the Variation is greatest when the sun is in perigee, and least when in apogee; and gives a rule for determining its value. But in the second edition of the *Principia* this rule is omitted: and the 7th equation is altered so as to serve as a correction for the Variation deduced agreeably to Prop. 29. See the notes to Gregory's Scholium, at the end of this Supplement.

“regarde le mouvement de l'apogée et la variation de l'eccentricité et toutes
 “les autres inégalités du mouvement de la lune, M. Newton se contente des
 “résultats qui conviennent aux astronomes pour construire leurs tables, et il
 “assure que sa théorie de la gravité l'a conduit à ces résultats. Horrox,
 “célèbre astronome Anglois, avoit prévenu M. Newton sur la partie la plus
 “difficile des mouvemens de la lune, sur ce qui régarde l'apogée et l'eccen-
 “tricité. On est étonné que ce savant, dénué du secours que fournissent le calcul
 “et le principe de l'attraction, ait pu parvenir à réduire des mouvemens si com-
 “posés sous des lois presque semblables à celles de M. Newton : et ce dernier,
 “si respectable d'ailleurs, paroît d'autant plus blamable en cette occasion d'avoir
 “caché sa méthode, qu'il s'exposoit à faire croire que ses théorèmes étoient,
 “comme ceux des astronomes qui l'avoient précédé, le résultat de l'examen des
 “observations, au lieu d'être une conséquence qu'il eut tirée de son principe
 “général*.”

But Newton did not confine himself in that document, solely to the above-mentioned ameliorations of the old tables; for he introduced 4 *new* equations, to be added thereto, but which are given *without demonstration*: and I apprehend that it is principally owing to Flamsteed's free and open opinions relative to these new equations, that he has been charged with ignorance of Newton's methods. One of these was comparatively very small, the maximum being only 47", depending on the position of the moon's node with respect to the sun, and is probably the equation alluded to in pages 137 and 138; another depending on the angular distance of the moon from the sun, the maximum of which (under certain circumstances†) was 2'. 20"; another depending on the same argument *minus* the moon's anomaly, or the angular distance of the moon's apogee from the sun, the maximum of which may be assumed as 3'. 45"; and another depending on the same argument as the last *plus* the sun's

* *Principes Mathématiques de la Philosophie Naturelle*, par Mad. la Marquise du Chastellet. Paris, 1759, vol. 2, page 104; a work which was revised, if not edited, by Clairaut.

† This equation did not depend solely on the angular distance of the moon from the sun, but was affected also by the relative positions of the solar and lunar apogees.

anomaly, or the angular distance of the moon's apogee from the sun's apogee, the maximum of which was $2'.10''$. Newton has not explained, in the document under review, how he deduced these new equations, nor whether any of them are derived from his own theory of gravitation, or from Horrox's theory of the libratory motion of the lunar apogee and the consequent variation of the eccentricity of the lunar orbit; indeed the whole subject of the motion of the lunar apogee is finally left by him in a very unfinished state: and, as he has not in this document given any demonstration, Flamsteed cannot fairly be charged with ignorance merely because he would not assent at once to the amount of these new corrections, and the propriety of their application. M. Bailly, in allusion to this subject, remarks in his lively style that "Newton avoit dit que sa théorie donnoit avec exactitude la quantité de ce mouvement; mais il avoit souvent parlé à la manière des prophètes, qui disent ce qu'on ne peut voir: alors c'est la foi qui croit, il faut que la raison se soumette. Mais l'esprit n'est ni éclairé, ni tranquille: son repos ne peut être établi que sur des démonstrations." *Hist. de l'Astron. Mod.* vol. 3, page 150. And Mr. Whiston, in one of his astronomical lectures at Cambridge, when he succeeded Newton as Lucasian professor there, has the following remark on the last of the above-mentioned equations, called by Newton the sixth: "How it should come to pass that this 6th equation of the moon should arise from causes, which are so unlike, joined together amongst themselves (as are the motion of the moon from the sun, and the motion of the apogee of the moon from the apogee of the sun), I must acknowledge myself to be altogether ignorant: nor is there opportunity for inquiring here in these matters merely astronomical. In the meanwhile, I suspect that this equation was rather deduced from Mr. Flamsteed's observations than from Sir Isaac Newton's own argumentation." We thus find the immediate successor in the chair of Newton apparently in doubt on some of the points of this new theory: at the same time ready to bestow equal justice on Newton and Flamsteed. See the first note in page 701.

In the absence of this necessary information, Flamsteed naturally considered

that the best proof of the truth of a theory, or rather of the deductions from such theory (for the theory may be correct, and the deductions erroneous), would be its accordance with observations; at least this has always been considered as the best test, from the time of Ptolemy to the present day: and the labours of Newton have not escaped this severe ordeal, witness the researches of Euler, D'Alembert, and Clairaut. See *Système du Monde*, page 381, fifth edition. It had been generally understood, and indeed Gregory had formally announced, that these new rules would give the moon's place true to 2 or 3 minutes, "ut dissensus à cœlo, etiam cum maximus, vix duo scrupula prima adæquet:" a degree of accordance at that time not much expected. Flamsteed therefore might justly doubt the accuracy of these assertions; for there was no one living that knew so well as he did, or that had such means of knowing, the true state of this portion of practical astronomy: and the result of his inquiries showed that the conjectures of Gregory and Halley were not well founded*. For he immediately computed a set of auxiliary tables, agreeably to the elements thus for the first time announced by Newton: or, as Flamsteed himself expresses it (page 216), "I fitted my new tables to Mr. Newton's additions to, and corrections of, my old and the Horroxian theory†. - - - I can now tell you that, in the first eclipses I calculated, they erred 10 minutes of time in the beginnings and ends: in some more, in others less, afterwards. You see what their great boast of answering my observed places of the moon within

* D'Alembert has justly remarked that these assertions of Gregory and Halley should be received with due caution, since they had no means of making any extensive comparisons: and that it requires accurate tables and a long series of observations in order to form a proper estimate of such subjects. See his *Recherches sur differens points importants du Système du Monde*, vol. 1. chap. 22.

† These new tables are the same as those described in pages 210, 211, and 212, which are still preserved at Greenwich in MSS. Vol. 50, I.; and which, by a date inserted at the end of the 25th table, were finished on Jan. 1, 1702—3, soon after the appearance of Dr. Gregory's book. Some of the fundamental tables, however, had been computed four years prior to that date. They are undoubtedly the same as those which were published 43 years afterwards by Le Monnier, as I shall mention more at large in the sequel (see page 705): and according to Mr. Crosthwait's evidence (page 335) they do not differ much from those published by Halley; which the reader indeed may ascertain by comparison.

“ 2 or 3 minutes, comes to : the calculations have been twice repeated.” Also in his letter to Mr. Caswell, in page 213, he says, “ I have answered all the particulars of your letter, and shall now tell you what I am doing. Soon after Mr. Newton’s book came out *, I made new tables for calculating the moon’s place according to all his additions and corrections of the old, on the Horroxian theory. But I applied them not till lately, when I set myself to examine them by 30 appulses of the moon observed here betwixt the years 1675 and 1689. I have repeated a dozen of them twice for greater certainty, and find the new numbers are commonly 5 or 6 minutes, sometimes in the excess, and in the opposite point of the orbit in the defect, and vice versâ ; and sometimes the errors rise to 8 or 9 in longitude ; and that, not in the quadratures, but at a distance from them. The errors in latitude are frequently 2, 3, or 4 minutes, which is intolerable. They result not only from my own observations, but from those of others taken at the same time. ’Tis much for the Doctor’s and his author’s *credit*, and shows how *skilful* they both are, that such errors are found in the first careful examination.” These caustic remarks seem harsh to our ears, but we must carry ourselves back to Flamsteed’s time (when Newton’s theory was but little understood and not fully developed,) for their justification, or at least for their palliation. Flamsteed had, indeed, pointed out the errors to Newton himself : but Newton would scarcely believe them, and parried the question. “ I showed him also (says Flamsteed) my new lunar numbers, fitted to his corrections, and how much they erred ; at which he seemed surprised, and said *it could not be*. But when he found that the errors of the tables were in observations made in 1675, 1676, and 1677, he laid hold on the time, and confessed he had not looked so far back †. Whereas if his deductions from the laws of gravitation were just,

* I presume that Flamsteed alludes to *Gregory’s* book : as Newton did not publish anything on the subject at this period.

† He had been actually furnished by Flamsteed with the lunar observations from January to July 1677, as appears by the last note in page 158. If therefore Newton had not discussed those observations, it would show that he had finished his lunar theory rather earlier than the date mentioned in page 687.

"they would agree equally in all times." See page 217. We shall find however, in the sequel, that Flamsteed was right: and as he had no other means of determining the accuracy or inaccuracy of these new equations, than this tentative method, since Newton had not given any demonstration, he persisted in his doubt of their propriety.

Flamsteed says (page 214), "I know how to take off part of the error, by throwing away part of Mr. Newton's equations, which the heavens allow not:" and he alludes in other places to his attempts at "rectifying the motions of the moon." For he had himself been, during the whole of a long life, engaged on this difficult and intricate subject: "no one (says he, page 211), would have been at the pains to make so many tables, as I have, for this purpose." And in these investigations he has not been inattentive to the newly discovered law of universal gravitation: for, in his letter to Mr. Bossley, page 172, he says, "I find further, from the old observations compared, that the moon's greatest latitudes agree not to the theory, but there is sometimes an error of near 2 minutes: but, it seems regular; which makes me hope, when I have leisure to think further of it, that I may both find a reason for it, from the laws of gravity, and a salve*." The new equations, however, introduced by Newton, gave him a good deal of trouble, as he could not reconcile them with the observations.

At length, on the appearance of the second edition of the *Principia* in 1713, where the Newtonian rules are given in an amended state, in the Scholium at the end of the 35th Proposition of the 3rd Book, Flamsteed detected that Newton had, in the copy sent to Gregory, made a mistake in the sign of his sixth equation (the last of those enumerated in page 693), and had also now made some alteration in his seventh (the second of those above-mentioned). "He has (says Flamsteed, page 304) lately published his *Principia* anew, wherein he makes this equation ablative where it was formerly to be added; and to

* See also his letter to Dr. Wallis, of June 24, 1701, in page 198; as likewise his letter to Mr. Sharp, of September 20, 1710, in page 277, and of May 2, 1717, in page 327; respecting the corrections of the motions of the superior planets.

“ be added, where it was subductive : and has altered his seventh so as in part
 “ to destroy it ; but I have *not yet* examined how this will answer*.” And
 afterwards he says, page 309, “ I told you that the heavens rejected that equa-
 “ tion of Sir I. Newton, which Gregory and Whiston called his sixth. I had
 “ then compared but 72 of my observations with the tables : now, I have exa-
 “ mined above 100 more. I find them all firm in the same†, and in the seventh
 “ too. And whereas Sir I. Newton has in his new book (pages 424 and 425)
 “ thrown off his sixth, and introduced one of near the same bigness but always
 “ of a contrary denomination, and a bigger in the room of the seventh, if I
 “ reject them both, the numbers will agree something better with the heavens
 “ than if I retain them : so that I have determined to lay these *crotchets* of Sir
 “ I. Newton’s wholly aside.” It was under such circumstances and feelings,
 and with a firm conviction of the inadequacy of two (at least) of Newton’s new
 equations, that we find Flamsteed giving utterance in his familiar letters to
 those trite yet candid expressions of his opinion, which have given so much
 offence to Newton’s admirers. And although such expressions sound harsh
 and uncourteous to our ears, yet they scarcely justify a charge of *ignorance* of
 Newton’s theory of Universal Gravitation.

Flamsteed however did not confine himself to the odious task of merely cen-
 suring Newton’s production, for he was also seriously engaged in endeavouring
 to amend these presumed errors. In his letter to Mr. Sharp, August 31, 1714,
 page 310, he says, “ There are still some circumstances in her [the moon’s]
 “ motions that have not been considered, which I am getting upon as soon as I
 “ can get help and leisure : for, my servant and Mr. Ryley are not enough.
 “ At present I shall only acquaint you, 1°, that I reject Sir I. Newton’s first
 “ equation of the apogee‡ ; though I fear I shall be forced to introduce

* In the third edition of the *Principia*, Newton has omitted this seventh equation altogether : the correction of the Variation being proposed in another manner. So that Flamsteed was not much mistaken in his estimate of the practical value of these two equations, as first propounded.

† That is “ in the *rejection* of the same.” F. B.

‡ This is the equation depending on the sun’s anomaly : of which, see what has been said in the note in page 692. The “ greatest equations,” presently mentioned, are those given in page 155.

“ another : 2° , that I add $1' 30''$ to the moon's mean motions : 3° , that I diminish the greatest equations of the apogee, &c. : 4° , that I neglect the alterations of the Variation : and 5° , allow a small equation that extends itself even to the quadratures.” He then adds that he is considering of a *new* equation that had not yet been thought of by Newton. So that we may, in some measure, judge by these extracts whether Flamsteed *understood*, and in what way and to what extent he understood, the subject under discussion. I am aware that it would be idle to pretend that Flamsteed investigated or was capable of investigating the subject in the same profound manner as Newton pursued it ; and there were probably few men, of that day, that were equal to such a task : Flamsteed's talents were of a different order. The important truths involved in Newton's general theory had not yet acquired that stamp of authority which they so justly obtained at a subsequent period : nor indeed were they then capable of being followed out to the same extent as at the present day. Flamsteed's labours, out of the observatory, were of a more humble nature, and consisted principally in the correction of the solar, lunar, and planetary tables *from observations* : and in this department there were few men equal to him in his day *. Even if Flamsteed did not cordially *assent* to Newton's general theory (of which we have not as yet seen sufficient evidence), it should not be considered as a reproach at that day, since there were many men, of much higher mathematical attainments, that had considerable doubts on the subject : and we find an ample apology, for such unbelievers, in Cotes's preface to the second edition of the *Principia* ; who, more lenient than the moderns, and knowing the difficulty of overcoming ancient prejudices and doctrines, has this liberal passage. “ Scio equidem nonnullos magni etiam nominis viros, præjudiciis quibusdam plus æquo occupatos, huic novo principio ægre assentire potuisse, et certis incerta identidem prætulisse. *Horum famam vellicare non est ani-*

* “ My age and infirmities (says Flamsteed, page 266) suffer me not to examine them [Jupiter's inequalities] as I would. All I can do is to lay in a good stock of observations, as I have done, for the primary planets ; whereby posterity may be enabled to proceed where I am forced to leave off, through the envy of ill men, lest I should impoverish my nearest relations, whom I am bound, for justice and conscience to take care of, since they are in no capacity to provide for themselves.”

"*mus.* Tibi potius, benevole lector illa paucis exponere lubet, ex quibus tute
 " ipse judicium non iniquum feras."

When we consider therefore how little of what is now termed Newton's theory of the moon was known *prior* to the second edition of the *Principia*, that it was (as I have before stated) confined as far as its practical application was concerned, to that portion of it, which (mixed up in an undistinguishable manner with the theory of Horrox), was communicated to Gregory, and a part of it being, in fact, erroneous in the detail : when we consider also that, even on the appearance of the second edition of the *Principia*, Newton himself continued in doubt as to the true amount of some of these new equations ; that he has, in fact, eventually discarded one of them, and changed the *sign* of another, and that the whole still remained but an incomplete theory (incomplete in its application) requiring the powerful aid of a long succession of the most distinguished mathematicians to unravel and elucidate :—it does seem to be bearing rather too hard upon Flamsteed to charge him with ignorance of the subject, because he would not immediately assent to a theory, or rather to a set of *rules*, a portion of which he found to be at variance with his own observations ; and more especially when such alleged ignorance is brought against him with the *useless* object of screening other persons, in disputes which have nothing whatever to do with the subject. Surely it cannot avail the cause of truth to draw such invidious distinctions between the respective talents of distinguished contemporaries, and to lower the character of one in order to raise that of the other. To use the appropriate words of Mr. Whewell, " I do not think it a reasonable infliction, either
 " on the reader or the writer, that a discussion of the character of one man
 " should ramify into controversies on the merits of several others." Where all are cordially and earnestly engaged in the common pursuit of truth (as was evidently the case with Newton and Flamsteed during the whole *progress* of this inquiry) the praise is equally due, although the talent may be disproportioned. But, if the scale of intellect alone is to be the test of merit, who shall be able to compete with the mind of Newton ?

Let us now see how this new lunar theory, or rather how this enunciation of

it by means of Gregory's *Newtonian rules*, (for the great truths in the body of the *Principia* had not yet been, nor were they for many years after, developed) was received by the public as a *practical* solution of the great problem of the moon's motions. In the year 1707, five years after the appearance of Gregory's book, we find that Whiston, the immediate successor of Newton as Lucasian professor at Cambridge, published his *Prælectiones Astronomicæ*, at the end of which he has reprinted Flamsteed's Horroxian lunar tables exactly as they are given in his *Doctrine of the Sphere*, but without any addition from Newton's theory*. Three years afterwards (1710), we find Halley himself revising the second edition of Street's *Caroline tables*, a work in very general use amongst astronomers: but not one word is there said about the amendments of Sir Isaac Newton, or the introduction of his four new equations†. In 1728 Whiston published a translation of his *Prælectiones Astronomicæ*, under the title of *Astronomical Lectures*, in which he again reprinted Flamsteed's Horroxian lunar tables, and still without any addition from Newton. In the year 1732, Wright published his *New and correct tables of the lunar motions according to the Newtonian theory*, where we find an imperfect attempt to embody Gregory's *Newtonian rules* into the existing lunar tables. The whole however is an abortive production: for, only the second of Newton's equations is distinctly introduced; whilst the third and sixth seem to be wholly omitted, and the seventh united to the Variation. Moreover, the maximum of eccentricity is quite at variance with Newton's assumptions: so that there is a partial adoption only, or rather a garbling, of Newton's rules. It was not, indeed, till the year

* He has inserted Gregory's *Newtonian rules*, with remarks thereon; but has not applied them, nor shown the application of them, to the construction of new tables. Flamsteed's opinion of this book may be learnt from the following passage which occurs at the end of his letter to Mr. Sharp, of July 1, 1707. "Mr. Whiston's *Prælectiones Astronomicæ* [is] a very poor and barren piece; save that, at the latter end he has added my *new* solar, and *old* lunar tables: and in the last page of these he tells the world what alterations or additions Sir Isaac Newton has made of those numbers; which is downright acquainting the world that his new theory of the moon is only Mr. Horrox's corrected by Sir Isaac. Had I said so much, it would have been looked upon as a piece of arrogance."

† In the Appendix, page 70, there is a slight allusion to Newton's "true and physical theory of the moon;" but no explanation of the data on which it is founded, nor of its application.

1735, when Leadbetter published his *Uranoscopia*, that we find a more perfect adoption of Gregory's *Newtonian rules* reduced to a tabular form; the whole of his four new equations being regularly introduced agreeably to Newton's directions*. From this period we find these *Newtonian tables*, and others similarly constructed, in common use together with Street's *Caroline tables* and Flamsteed's *Horroxian tables*, and without any decided or known preference; till they were all supplanted by those more correct ones, founded on the laborious investigations of Euler, D'Alembert, Clairaut, Mayer, and others.

Thus it appears that a period of more than 30 years elapsed before Gregory's *Newtonian rules* were thrown into the form of tables for public use; or any notice taken of Newton's theory (amended as it was in the second edition of his *Principia*) in the construction of lunar tables for the public: and moreover that a still further period of 20 years was suffered to pass away before Horrox's theory, thus combined with Newton's, was entirely abandoned; no person, previous to the expiration of that period, having been bold enough to undertake the arduous task of developing Newton's theory, and constructing tables founded *solely* on his principles. The appearance of Mayer's *Nouvelles tables de la lune* in 1753, upwards of 50 years after Gregory's book, may be considered as the first lunar tables thus formed, and *confirmed by observations*: since he deduced all his Arguments of the lunar inequalities from Newton's physical theory alone, and determined the numerical values from astronomical observations.

On this change in the mode of developing the lunar theory, M. Bailly has the following pertinent remarks, in his *Hist. de l'Astron. mod.* Vol. 2, page 508.

“ Nous n'entrerons point dans le détail de la manière dont les différentes équations du mouvement de la lune s'expliquent par la théorie de la gravité. “ Newton n'a fait qu'ébaucher cette partie: ce sont les géomètres du siècle

* The variation of these new equations, according to the direct distance of the *sun* from the earth, and of the earth from the moon (as pointed out by Newton), does not appear to have been fully attended to in the formation of the lunar tables, either by Leadbetter or any of his contemporaries. And it is worthy of notice that Leadbetter published a second edition of his *Astronomy* in the year 1742, and inserted lunar tables founded on the old system, without any allusion to Newton's labours.

“ présent qui l'ont portée à sa perfection.—Cette partie est celle où Newton s'est
 “ enveloppé de plus d'obscurité. Il semble avoir caché la route qu'il a suivie :
 “ on voit qu'il n'a point embrassé dans son entier le problème de déterminer
 “ les inégalités de la lune, en consequence de la force perturbatrice du soleil : il
 “ a voulu montrer seulement en général, et par quelques exemples, que ces
 “ inégalités pouvoient se deduire de la théorie de la gravitation. Il paroît
 “ adopter l'hypothèse d'Horrox, renouvelée par Halley, qui fait mouvoir le
 “ centre de l'ellipse de la lune dans un petit cercle, pour éloigner ce centre et
 “ l'approcher de la terre, pour faire varier l'excentricité, et expliquer la seconde
 “ équation de Ptolémée, qui a lieu dans les quadratures*. Cette hypothèse est
 “ ingénieuse, mais ce ne peut être jamais qu'une hypothèse. Newton sans
 “ doute ne l'eut point admise, s'il fût entré plus avant dans la théorie de la lune :
 “ il l'a laissé subsister comme une vraisemblance qui peut faire attendre la
 “ vérité, et tenir sa place.”

I would here remark that Flamsteed was in the habit of constructing and reconstructing tables of various kinds, solar, lunar, and planetary, according to the prevailing theories of the day, in order to ascertain their accordance with observations †. With Horrox's theory he was perfectly familiar: he had also a theory of *his own*, or rather an improvement of Horrox's theory; the only account of which is to be gleaned from what he has written to Mr. Sharp in his letter of January 18th, and February 9th, 1702-3, (pages 210—212); wherein he alludes to his having formed lunar tables agreeably to that theory ‡. These tables he extended, as we have already seen, so as to include Newton's new

* This revival of Horrox's hypothesis, which is here attributed to Halley, really belongs to Flamsteed, who was the first to publish the fact of the variation of the eccentricity of the lunar orbit, and to show how “the second equation of Ptolemy” (that is, the Evection) was deduced therefrom. The only suggested improvements of Horrox's theory by Halley (at least, as far as any *published* accounts inform us) are those mentioned in the note in page 684, where he merely proposes to deduce the Variation, but has not followed out his plan; and the alteration mentioned in the note in page 683. It is probable that M. Bailly was led to this view of the subject from mistaking what is stated by Newton in the Scholium to the 35th Prop. of the 3rd Book of the *Principia*.

† See especially his letters to Mr. Sharp, of July 14, and Sept. 20, 1710, in pages 276 and 277.

‡ I use the term *theory* here in the sense in which it was used in Flamsteed's time; where it meant empirical or *tabular*, rather than *physical* theory. See page 691.

equations, as given in Gregory's *Newtonian rules* ; and which he had intended to publish, together with some others, in one of the volumes of his *Historia Cœlestis*. It appears however that, from some cause or another, this was not carried into effect*. On this subject let us hear what Mr. Hodgson, who married Flamsteed's niece, has recorded in the Introduction to his *Theory of Jupiter's Satellites*, printed in 1750. " And here (says he) I cannot help " taking notice of a great hardship that I labour under ; and that is this. " Mr. Flamsteed, under whom I had the happiness of my education, was pleased " to set me upon computing his lunar tables, under his direction ; when I com- " puted the tables of central equations of the moon after the Keplerian method, " which had never been done before. And in consideration of the labour I had " taken in this, as well as in calculating the latitudes and longitudes of all the " stars in the British Catalogue, which amount to upwards of 3000, and in " other innumerable calculations, and some other motives, he was pleased to " appoint me one of his executors ; and at the same time thought proper to " recommend to me the care of completing and publishing what he had left " undone ; which I have performed with all the integrity in my power. But, " as to the lunar tables, the publication of them was delayed for very good " reasons ; and now to my great surprise I find them printed in M. Le Mon- " nier's *Institutions Astronomiques* : but how he came by them is to me at " present a mystery. Mr. Flamsteed was, in himself, a man of very commu- " nicative temper to those he took a liking to, and whom he thought he could " trust : and I know two persons to whom he gave each a copy. But now, " after upwards of 20 years, when it was well known that I had the original " by me, and did at a convenient time intend to send them into the world " according to Mr. Flamsteed's own direction, it was base and disingenuous (to " say no worse) in them, whoever they were, and who had no right to them, to " betray their trust, and deprive me of the satisfaction of complying with the " request of my friend, and printing them according to his own mind."

* See Mr. Crosthwait's letter of May 6, 1720, in page 335. In that letter it is stated that Flamsteed's lunar tables had been entered at Stationers' Hall : but I have examined the Book of Entries there, and have not been able to discover that such was the fact. There is only one book entered by Flamsteed ; and that is a Tide Table.

Besides these lunar tables in the *Institutions Astronomiques* of M. Le Monnier, which appeared in the year 1746, and which are evidently copied from Flamsteed's tables still existing at the Royal Observatory (see the note in page 695), as I have recently ascertained by a minute examination and comparison *, Halley also formed similar tables derived from the same *Newtonian rules* as corrected in the second edition of the *Principia*; which, although constructed in 1717, and printed in 1719, were not published till 1749. He appears to have printed them about the time that he was appointed Astronomer Royal; and they were probably constructed for his own private use, as Flamsteed had done before him. M. De Lisle, in his *Lettres sur les tables astronomiques de M. Halley*, 1749, remarks that when he was in England in the year 1724, Halley gave him a copy of these tables; but he adds, "ce ne fut qu'après l'avoir assuré, comme il exigeoit de moi, *que je ne les communiquerois à aucun astronome, et que je m'en réserverois à moi seul l'usage que j'en feroi.*" In his second letter (1750) M. De Lisle says that he himself was the *first* person that calculated lunar tables from Newton's principles: and that there was a manuscript copy of such tables placed by him in the library of the Duc de Noailles, in the year 1717, which would prove the fact. He gives no reason for the suppression of this circumstance in his first letter: and however it may be, it is very clear, from this short history of the subject, that the lunar tables founded on the *rules* given by Newton, were not held in much greater estimation than those which had previously existed †: and that it was not till the subject was developed anew by his successors, on the sole principle of Universal Gravitation, that the public could fully appreciate all the merit to which the theory is so justly entitled ‡.

* M. Le Monnier has added, in the tables of the Equation of the Centre, and of the Horizontal Parallax, an additional column for the *greatest* eccentricity: a measure which was quite unnecessary if he had fully understood the mode in which Flamsteed constructed his MS. tables. For Flamsteed's tables had collateral columns of proportional parts to the different eccentricities, which Le Monnier has wholly omitted.

† See Mr. Crosthwait's letter of May 6, 1720, in page 335.

‡ The reader, who is desirous of entering more fully into the historical detail of the lunar theory at this period, should consult D'Alembert's *Recherches sur differens points importants du Système*

Before I leave this subject I would make one concluding remark, which may perhaps enable the reader to form a still more correct opinion on the point at issue, with which I originally set out: namely, that there does not appear to be any evidence that Newton resumed the subject of the lunar theory, after he had furnished Gregory with the results of his inquiries, in or prior to the year 1700. The investigation had evidently taken him longer than he expected; since he imagined, at first, that a few months only would be requisite. In his first letter to Flamsteed, page 133, he says, "By such a set of observations I believe I could set right the moon's theory *this winter*:" and in a subsequent letter, page 153, he says, "When I am about other things (as at present) I can neither fix to them with patience, nor do them without errors; which makes me let the moon's theory alone at present, with a design to set to it again, and *go through it at once*. When I have your materials I reckon it will prove a work of *about three or four months*: and when I have done it once, *I would have done with it for ever*." This last intention he seems to have kept: for I cannot find, in the second edition of the *Principia*, the trace of any further alterations or improvements in the lunar theory, than such as would have been, and probably were, suggested by the investigations which immediately preceded the communication to Gregory. We may therefore fairly suppose that, with the exception of some slight corrections which from time to time occurred to him (and some of which had evidently been pointed out by Flamsteed), he finally closed his inquiries on this subject, as I have already more than once remarked, before the year 1700. Indeed, Newton's occupations at the Mint during the great re-coinage of the silver currency (1696 and 1697) engaged much of his attention about this period; and he himself has stated that he would not be thought to be *trifling* away his time about mathematical things, when he should be about the King's business. See page 166*.

du Monde, 3 vols. quarto; and more especially the 3rd volume, published in 1756. The learned author prefers the lunar tables of Le Monnier (really Flamsteed's, see page 705, and the note in page 695) to those of Halley: but they are in fact both deduced from the same *Newtonian rules*.

* This famous re-coinage was performed not only in London, but also at Exeter, Bristol, York,

Should this be the case, it will show us that, in the dispute which took place *ten years afterwards* with Flamsteed, the publication of the Greenwich Observations was not required for the purpose of enabling Newton to work out the theory of the moon: more especially when it is also shown (as I shall presently do) that Newton had already been furnished with *every* observation that he had requested for that purpose: and moreover that there is no evidence that the observations, when published, were ever applied to the verification of Newton's lunar theory, by any of his contemporaries. In fact, they could not be so applied by the public, since (as I have already shown) there were no published tables, founded on his theory, with which the observations could be compared. Such a notion, indeed, is quite of modern origin; for there is no mention in any of the documents that Newton entertained or expressed an opinion of the kind; nor is there the most remote allusion to such an idea. It is evident therefore that the proposed publication, in 1711, of the Greenwich Observations made with the mural arc, had nothing to do with the lunar theory of Newton: the *crisis* of that important work having long passed by, and no other existing observations being *then* required for its further development; or, if required, Newton (as I shall presently show) actually had them all in his possession. On the contrary, it was Halley's opinion (more than once expressed), and an opinion which seems to have met with considerable approbation and support in his day, that the irregularities of the moon could be best discovered, and the corrections deduced, from a regular series of comparisons made during successive revolutions of the apogee and node; or in a cycle of 223 lunations, or about 18 years and 11 days. Astronomers, therefore, rather looked forward to *future* observations, to enable them to effect a reformation in the lunar tables, than to deduce the true law, at once, from the principles laid down by Newton. Le

Norwich, and Chester: to which latter place Halley was appointed by Newton to superintend the Mint there. And there are, amongst the Portsmouth MSS., several letters from Halley relative to some disputes in which he had become embroiled with the persons connected with that establishment. Newton was made Warden of the Mint in March, 1696: see page 63. And it was about this period that Flamsteed was told that he had *perfected* the lunar theory: see page 72.

Monnier, more especially, adopted this plan; in which he was followed by Cassini de Thury *: indeed it was pointed out by Newton himself so late as the year 1720, as one worthy of adoption †; and was in part pursued by Flamsteed. See pages 302 and 304.

I now dismiss this portion of the subject, and shall leave it to the public to decide whether, on a review of all these circumstances, Flamsteed did, or did not understand such portion of Newton's lunar theory as Newton thought proper at that time to propound and divulge for the use of the practical astronomer: and whether he was, or was not fully aware that in the investigation of that, or of any other theory, the greatest assistance was to be derived from astronomical observations.

The second point, on which I have thought it requisite to make a few remarks, is the implication that Flamsteed showed an unwillingness, and even an objection to furnish Newton with the necessary lunar observations to enable him to complete the theory of the moon; and that, in fact, Newton could not obtain from him all the observations that he requested.

I must confess that I have been somewhat surprised that such an opinion should have gained currency; and can only attribute the origin of such an assertion to an inattention to the order and connexion of the events that occurred within the limited period to which the inquiry need be restricted: and I ought rather to call for evidence of the correctness of the charge, before entering on any attempt at Flamsteed's defence. Newton resumed his investigations of the lunar theory in October 1694, and had finished his labours on this subject, as we have already shown, prior to the year 1700; probably about the end of the year 1698. The whole of what we know respecting the assistance that he required and obtained from Flamsteed, is to be found in the correspondence in the preceding pages, 133—160; which extends from October 7, 1694, to September 14, 1695. But I cannot there discover any trace of unwillingness or

* See Delambre's *Hist. de l'Astron. au xviii. siècle*, page 282.

† See *Memoirs of the Roy. Astron. Society*, vol. viii. page 171.

objection whatever to furnish Newton with all the observations that were, from time to time, called for by him. On the contrary, it is there shown that Flamsteed not only freely communicated every observation that Newton required, but that those which he did communicate were in fact *all* the lunar observations that were made by Flamsteed during the respective periods pointed out by Newton as peculiarly applicable to his purpose. Indeed, Newton's satisfaction with Flamsteed's conduct is specially acknowledged in some, and virtually in all, of his letters: and although there is an appearance of discontent (and, in my opinion, a want of courtesy) in Newton's letters of June 29 and July 9, 1695, because Flamsteed did not at that time supply him quick enough with some recent observations (Flamsteed having been for some months previously laid up with a fit of the stone, and a violent headache, to which he was constitutionally liable), yet, on an explanation given, the correspondence is renewed to the mutual satisfaction of both parties. It is true that, if we may judge from the rough draft of some of Flamsteed's answers, there are two or three out-breakings relative to Halley, the constant object of Flamsteed's dislike, and whom he suspected (whether rightly or not, is a matter of no consequence in the present view of the case) of a design to obtain the observations from Newton, for some purpose of his own: but he appears to have been satisfied with Newton's explanations, and with his assurance that they should not be communicated to any one without Flamsteed's permission. But that he refused, or even objected, to send the observations to Newton himself, and for the purposes required, there is not (as far as I can discern) the least particle of evidence: on the contrary, he appears to me to be extremely anxious, and even impatient (see page 158), that Newton should undertake and finish the arduous task of "settling the moon's motions." In the rough draft of an answer sent by Flamsteed to Newton, he says, page 150, "I shall mind my business of the fixed stars, and give him an account of my progress, whilst he is employed on the moon: and shall be *very well pleased* with an account of his success." But it is needless to multiply extracts, in order to refute a charge, of the accuracy of which I cannot discover any evidence.

The correspondence seems to have closed on September 14, 1695*, with a letter from Newton; to which it appears that Flamsteed had sketched an answer (page 160), stating amongst other things that "when you want more of my lunar observations, I shall cause them to be transcribed, and it will be no trouble." This correspondence therefore ended, as far as we have any evidence, with mutual feelings of kindness and friendship. How that friendship was afterwards broken, and terminated in hostile recriminations, must be looked for in the *subsequent* proceedings of the parties, *long after the settlement of the lunar theory*: and those disputes must not be mixed up with this correspondence.

So much then for Flamsteed's unwillingness to furnish the lunar observations. I shall now proceed to show that there is every appearance that he forwarded to Newton *all* the lunar observations that he made, at the respective periods pointed out by Newton as the most favourable for the investigation of the lunar theory. I think we have sufficient evidence of this from the tenor of the correspondence just alluded to, and from the statement which Flamsteed himself has recorded on the back of one of Newton's letters, page 142 †: but I shall descend to further particulars in support of the argument, and bring forward other evidence to substantiate the fact.

The first list that was communicated to Newton contained about 150 com-

* There is one more letter, dated on December 29, 1698, three years after the close of the correspondence mentioned in the text: and it is in this letter that we trace the first symptom of coolness between these parties; for, in the margin of the book, from which that letter is copied, Flamsteed remarks, "he is *reserved* to me, contrary to his *promise*: I lie under no obligations to be open to him." This was written on the eve of that extraordinary letter which Newton sent to Flamsteed (see page 166), and which eventually led to an open rupture between them; although for a long time afterwards the outward forms of civility were preserved. The *reserve* above mentioned evidently refers, not to Newton's humour, but to his backwardness in communicating his lunar theory; for it stands here coupled with his *promise*. See, in confirmation of this, Flamsteed's letter to Lowthorp, in page 174.

† This statement, in page 142, is (through some confusion in the copy of the amanuensis) said by me to have been written by Flamsteed on the back of Newton's letter of November 17, 1694, but I subsequently found that it was written on Newton's letter of June 29, 1695, in page 157: where indeed the error is pointed out, and corrected.

puted places of the moon, with the differences or errors of the tables, arranged in three synopses. See pages 57 and 61. These synopses were evidently those which are mentioned in Flamsteed's list, in page 142, and contain, in reality, 157 places of the moon obtained with the mural arc from November 16, 1689, to April 16, 1692. In a letter dated February 7, 1694-5, Flamsteed sent 6 more places, obtained between the preceding date, and June 16, 1692. So that Flamsteed had then forwarded to Newton 163 computed places of the moon, deduced from observations made between the date of his first setting up the mural arc, to the 16th of June, 1692. Now, if we examine the second volume of the *Historia Cœlestis*, we shall find that these comprise *the whole of the lunar observations made by Flamsteed within that period* *.

It appears that after Newton had obtained this regular series of 163 computed places of the moon, with the errors or differences of the tables then in use annexed thereto, he was the better enabled to point out the positions of the moon, where the greatest corrections were required. Flamsteed says in his letter to Dr. Wallis, page 197, "Mr. Newton had done nothing in the theory of the moon, if I had only given him the *observations* here made: I was forced to give him the *places computed*, by myself and servants, from them, and repeated carefully: as also her places computed in like manner, and repeated from my own tables (grounded on the Horroxian theory) *with all the elements of the calculation*; whereby he was showed at once in what parts or positions of her orbit, in respect of the sun, the notable errors happened; and, comparing them with such as ought to be according to his theory of gravitation, how they might be taken away." Newton accordingly took advantage of this circumstance, and applied to Flamsteed to furnish him with the observations, when the moon was in any of such positions. Thus, in the very first letter, October 7, 1694, page 133, he says, "Your observations of this winter will be very material;" and he proceeds to point out more especially 2 or 3 observations about the first and last quadratures in October. The com-

* There are 2 or 3 *imperfect* observations in that interval, which of course are not reckoned in the number.

puted places of the moon for the period here indicated, were duly sent to him ; as appears by the list in page 142. And on examining the second volume of the *Historia Cælestis*, it will be seen that the several lunar observations above mentioned, from September 15, 1694, to January 18, 1694-5, being 26 in number, are in this case also *the whole of the lunar observations made by Flamsteed within that period**.

In the letter of November 17, 1694, page 139, Newton extends his request to have the observations for the last six months ; thereby including the months of May, June, July, and August, in addition to those already asked for : and he points out the precise mode in which he would have the *naked observations* arranged ; for hitherto Flamsteed had preferred sending the *computed places*. This slight circumstance enables us to ascertain that Newton did not apply in vain : for in his letter of December 4, 1694, page 143, he says, “ I thank you “ for complying with my request of sending me the *observed right ascensions* “ *and meridian altitudes of the moon* ; and for the *catalogue* you have given me “ of your *observations*.” This acknowledgment is of the more importance, as the communication of *these* lunar observations does not appear on Flamsteed's list in page 142.

The “ catalogue of observations” mentioned in that letter, evidently denotes a *list of dates* when Flamsteed had made any lunar observations ; and which list he had forwarded to Newton, in order to enable him to point out such as might

* I ought not to overlook Newton's request, in the same letter, to have the observations which had been made in the months of March, June, September, and December, for the last 6 or 7 years, when the moon was in a particular position of her orbit : the compliance with which I cannot find anywhere directly acknowledged. It is evident that this period would include about 3 years of the sextant observations ; viz., from 1687 to 1689, both inclusive : those made with the mural arc, from 1690, to June, 1692, inclusive, having already been transmitted in the synopses. Now, on examining the first volume of the *Historia Cælestis*, it appears that Flamsteed had made only 6 observations in those months, during the whole of that period : and as it is uncertain whether those observations were made when the moon was precisely in the positions pointed out by Newton, no great stress can be laid on this omission, even supposing such to have been the case. But, as Flamsteed, in his answer to Newton, page 134, says, “ I shall endeavour to satisfy you within a week's time,” the fair conclusion is that they were transmitted, if found on examination to be of the description required : for Newton only wanted them under certain conditions. See the following note.

have been made, when the moon was in those positions of her orbit that were most fit for the elucidation of certain parts of the theory *. For, Newton immediately adds, “ If you please to send me those of August, September, and “ December, 1692, and those of January, March, April, and October, 1693, and “ all those of the year 1694, except the three first (that is, the observation of “ January 25, and all those that follow), you will oblige me. Also in the year “ 1693 add the observations of September 30th and November 2nd.” We therefore find Flamsteed employed not only in providing Newton with such observations as he requested, but also in anticipating his wants by furnishing him with a list, from which he was enabled to select those which were most fit for his purpose. Newton proceeds also in the same letter to request Flamsteed to make some special observations during the ensuing lunation; for he says “ Pray, this next moon, make all the observations you can, and begin your “ observations when the moon is in the first octant if you can: for, the position “ of the apogee in the sun’s opposition in mid-winter is a case of great moment, “ and will not return in many years.” Let us now read Flamsteed’s reply to this fresh demand; and first, as to the ensuing lunation: “ If it proves frost (says “ he) I promise myself fair weather and frequent opportunities of determining “ her [the moon’s] place in the meridian, *which you need not doubt but will be “ imparted to you.* But, I must intreat you to be patient and bear with me for “ a little time: for, I must visit my cure at Christmas, and prepare before for “ my journey to it, which will employ me some days: so that I cannot give you “ the *places* of the moon you desire *till after the holidays.* But, *then you shall “ have them,* if God spare me life and health; and without any consideration “ or recompence but such communications as are usually made betwixt persons “ conversant in the same sort of studies.” Now the observations made in the

* That this was Newton’s mode of proceeding is clear from many passages: indeed, he himself says, in the *Principia*, “ *theoria vero lunæ, primo in sizygiis, deinde in quadraturis, et ultimo in “ octantibus, per phænomena examinari et stabiliri.*” It was only certain classes of observations therefore that he required: and this is evident from various passages in his letters to Flamsteed. See also *Memoirs of the Roy. Astron. Soc.* Vol. 8, page 171.

lunation here mentioned were sent to Newton, and duly acknowledged by him in his letter of February 16, 1694-5: and that the other collection of observations, requested by Newton, was also sent to him, we have the assertion of Flamsteed in the rough draft of the answer to Newton's letter, in page 143; where he states that being ill with the head-ache, and therefore not able to calculate, he "sent him the *observations* that he might compare the moon's *places* from them himself." And although there is no express acknowledgment afterwards from Newton that he had received those observations, yet I think it may be fairly left to the reader's unbiassed judgment whether, after this volunteer offer on the part of Flamsteed, and the direct assertion of its performance, there can be the least doubt upon the subject. I find, on examining the second volume of the *Historia Cœlestis*, that the required observations above mentioned amounted to only 12 during the year 1692, to 17 during 1693, and to 45 during 1694; out of which last-mentioned 45, we know from Flamsteed's statement in page 142, that *nineteen at least had been already communicated*: so that only 26 remain unaccounted for in that year. Indeed, there can be no valid excuse for his not having forwarded this comparatively small number; and we may therefore reasonably conclude that it was accordingly done; more especially as Flamsteed has asserted the fact, and as the subject is not again alluded to.

In a letter dated January 26, 1694-5, Newton requests to have some further observations; for he says, "Three or four observations at the end of this moon, and as many opposite to them at the beginning of the next, would be very significant." Now, it so happened that Flamsteed did not make any lunar observations at the *end* of the moon here alluded to, and that he made only two at the *beginning* of the next moon: which two, viz., February 8 and 14, were sent to him, as appears by the list in page 142.

In this letter Newton expresses himself grateful (as, indeed he had done in former letters) for what Flamsteed had done for him; and proposes to send him a new table of refraction, in order, says he, "that I might have something to present you with, for the pains that you have taken for me about your

“observations.” In fact, throughout the whole of this correspondence, there is a kindly feeling and a mutual disposition to assist each other. Newton had not asked for a single observation with which Flamsteed had not shown the greatest readiness to supply him, and even to anticipate him: and it was just at this friendly period that Newton's next letter, of February 16, 1694-5, was written; which (strange to say) seems to have created, in the minds of some persons, such unfounded and unnecessary alarms at the risk to which Newton's lunar theory was imagined to be exposed on account of the *unwillingness* of Flamsteed to furnish his lunar observations*.

Newton does not appear to have asked for any further observations till his letter of June 29, 1695, to which I shall presently draw the attention of the reader. For, although there are three intermediate letters on record, namely, one dated March 15th, another dated April 23rd, and the other dated April 25th, yet in neither of them is there any allusion to a request for observations. It is requisite to bear this fact in remembrance, for the reasons which will presently appear.

I have thus brought the reader, step by step, from the commencement of this correspondence, up to the middle of the year 1695; analysing every letter in which there was a request for observations. During the whole of this time I have shown that not only we do not find a word of complaint from Newton that

* The subject of this letter seems to have been imperfectly understood. For the better elucidation of it, it must be taken in connexion with the notes which are appended thereto. See page 152. Flamsteed never showed any unwillingness to communicate his lunar observations to *Newton*; but he had a great objection (whether well or ill founded is not the question) to their being shown to *Halley*; and probably also to their being *then* published, as they had not yet been corrected by his new catalogue of stars, then under reduction. Moreover, Newton's remarks applied only to the *lunar* observations, and not to Flamsteed's general astronomical observations, of the printing of which there was at that time *no prospect*. There is also another circumstance recorded (in the note in page 160), which clearly shows Flamsteed's feelings on this subject. Halley had employed a friend to procure from Flamsteed some observations of the comet of 1683 which were requisite in the computation which he was then (unknown to Flamsteed) making for Newton. In allusion to this request, Flamsteed thus writes to Newton: “Halley's behaviour towards me has been the most impudently and ungratefully base. I know him and you do not; therefore am resolved to have no further concern with *him*: but if *you* want any [observations] of that comet, I shall give *you* them, and leave to employ them as you please.”

he was not furnished with the requisite observations, but, on the contrary, expressions of thankfulness and gratitude for the favours bestowed by Flamsteed: whilst, on Flamsteed's part we see the greatest willingness to assist in the important work of perfecting the lunar theory, and a desire even to anticipate the wants of Newton*. The next letter, however, in the order of the series, casts a temporary cloud over this flattering picture: it is Newton's letter of June 29, 1695, above mentioned, which contains the following passage (see page 157): "I received your solar tables, and thank you for them. But these
 "and almost all your communications will be useless to me, unless you can
 "propose some practicable way or other of supplying me with observations.
 "For, as your *health* and other business will not permit you to calculate the
 "moon's places from your observations, so it never was my inclination to put
 "you upon such a task, knowing that the tediousness of such a design will make
 "me as weary with expectation as you with drudgery. I want not your *cal-*
 "*culations* but your *observations* only." The tone of this letter was evidently uncalled for, and is unworthy of the high character that wrote it. Flamsteed's excuse must be given in his own words: "I was ill (says he) all this summer,
 "and could not furnish him as I had formerly done." Newton, it seems, *was aware of this*. Flamsteed felt, and keenly felt Newton's rebuke; but he did not resent it: on the contrary, he immediately forwarded to Newton *the whole of the lunar observations that he had made, from the date of his last communication to the very day on which he answered the letter*. They were sent in an *uncalculated* state, agreeably to Newton's wish, in order to save time; although Flamsteed always preferred sending the *computed* places: a list of them, 30 in

* It has been publicly stated more than once that, during this correspondence, the letters of Flamsteed were written in a tone of *increasing* coldness and even acrimony. Now, the fact is, that there are only *two* letters from Flamsteed to Newton; in neither of which is there a single angry expression. Flamsteed's caustic notes on some of Newton's expressions are restricted to Newton's two uncourteous letters of June 29, and July 9, 1695, to which I am now about to allude; and was the record of his own private feelings only, and not intended to be made known to others. His reflections on this subject, in after life, as expressed in various parts of his own history, have doubtless received a colouring from the subsequent treatment which he experienced.

number, may be seen in page 142 *. And here it is worthy of remark, and should be borne in mind by all those who have the least suspicion that Flamsteed was unwilling to communicate his observations, that by this last communication *he had exhausted the whole of his lunar observations made with the mural arc*, in those positions of the moon pointed out by Newton: so that in fact he had literally no more of such observations to communicate. The mural arc was set up in the latter end of the year 1689: and we have already seen that from that period to the middle of 1692, the *whole* of the lunar observations (whether appropriate or not to Newton's purpose) were communicated to him. From the middle of 1692 up to the date of the letter under consideration, Newton had, from time to time, selected and obtained such as were most suited to the subject of his inquiry. If therefore Newton wanted any *more* observations, it was necessary to have recourse to a secondary class, or to the *sextant* observations, made *prior* to the setting up of the mural arc: to this subject, however, I shall again advert presently.

But I have not yet done with the unpleasant subject of the preceding letter: for I shall proceed to show that no blame can reasonably be attached to Flamsteed for the apparent neglect, thus so unceremoniously censured; even though he were *bound* to furnish Newton with the observations, instead of sending them as a *favour*: since it will appear that Flamsteed had an implied license for the delay. On the 23rd of April, only two months previously, Newton wrote to Flamsteed as follows: "When I set myself wholly to calculations (as I did for " a time last autumn, and again since Christmas in making the table of refraction), I can endure them, and go through them well enough: but when I " am about *other* things, *as at present*, I can neither fix to them with patience,

* The observation of April 16, is inadvertently omitted: and that of April 25, appears to be an exception; but it will be seen, from the note in the Preface, page xxxv, that this last-mentioned observation was one of those for which Newton paid a visit to the Royal Observatory, in order to get the correct computed place. The observation of June 13, which is not inserted in Flamsteed's list, was an imperfect one. The observations of February 25, April 9 and 15, and May 24, are not printed in the *Historia Cœlestis*. February 3, seems to be a misprint for February 8. Thus I have accounted for the whole set alluded to.

“ nor do them without errors ; *which makes me let the moon's theory alone at present*, with a design to set to it again, and go through it at once.” And again, two days afterwards, in his letter of April 25th, when speaking of the difficulty of determining the annual irregularities of the moon, he says that they “ observe such laws as I cannot yet determine : *nor have I been considering this point since I wrote to you last.*” Surely in the common intercourse of friends, this might fairly be assumed as an indication that Newton was not in any immediate haste for the observations : and Flamsteed might justly be surprised (and probably irritated), that within the short space of two months after the receipt of such remarks, Newton should become so suddenly impatient as to write such a peremptory letter as that we have just been considering ; followed immediately by another of a similar tendency, and which Flamsteed has designated as “ a hasty, artificial, unkind, and arrogant letter.” See page 158.

Flamsteed, however, returned good for evil : for, it appears that, with a view of furthering and promoting the object in which Newton was engaged, he not only sent him the observations above required, which, as I have just noticed, exhausted his stock of *mural arc* observations, but also offered to transcribe all his *sextant* observations of the moon from 1679* to 1690 : being nearly all the lunar observations he had made since he was appointed Astronomer Royal. To which Newton in reply says, “ I thankfully accept of your offer, and will “ get as many of them computed as are sufficient for my purpose.” Flamsteed immediately sent him the first instalment of those observations, namely, those that were made in the interval from January to July 1677 ; and are probably those the receipt of which is acknowledged by Newton in his letter of July 20, 1695. The former amicable and friendly correspondence is then renewed between them, and at the same time a kind of apology is offered for what had passed. “ The other day (says Newton, in his letter of July 27, 1695), I had

* I apprehend that this date should be 1677 ; for it appears, from what immediately follows, that this was the year contemplated by Flamsteed, and indeed acted upon by him, since Newton afterwards requests him to continue his observations till October 1677. Professor Rigaud however has been kind enough to re-examine the MSS., and finds that the respective dates are as here stated.

“ an excuse sent me for what was said at London about *your not communicating*,
 “ and that the *report* should proceed no further: I am glad all misunderstand-
 “ ings are composed*.” He then proceeds, in the same letter, to ask for further
 old sextant observations which he considered requisite in his investigations;
 which from the tenor of Flamsteed's answer to the next following letter, which
 I shall presently quote, were evidently then sent, or proposed to be sent, when-
 ever Newton was ready for them. The conclusion of the letter is worthy of
 remark, since it shows that however great might have been Newton's *haste*, a
 few weeks previously, for a continuance of the observations, he was now in the
 fair course of being supplied beyond his immediate wants: for he says, “ I shall
 “ not have time to go through *all* your observations, but will send you the times
 “ for which I would have them, *when I have done with these*, for this position
 “ of the aphelium.” In fact, in the very letter of acknowledgment of the
 receipt of the observations (July 20th), there is a clear indication that such
haste was not immediately requisite: for Newton says, “ I thank you for your
 “ communications of the table of fixed stars, and your lunar observations. *So*
 “ *soon as I have got some business off my hands*, I intend to get such of them
 “ calculated as I have need of.”

The last letter in the series is dated September 14, 1695, and is to the same
 purport: for Newton says, “ I am newly returned from a journey I lately took
 “ into Lincolnshire, and am going another journey: so that *I have not yet got*
 “ *any time to think of the theory of the moon; nor shall I have leisure for it*
 “ *this month or above*: which I thought fit to give you notice of, that you may
 “ not wonder at my silence.” The rough draft of Flamsteed's reply, which
 closes this interesting correspondence, shows that it terminated on friendly
 terms: for he says, page 160, “ My distemper abates: the pains of my head are
 “ not greater; but I am rarely free from them but when I am travelling. I

* The *report* here referred to is mentioned by Newton in his preceding letter; and alludes to the
 subject of the misunderstanding just recorded: at least, this is my interpretation of the case. But,
 others have imagined that it has reference to Flamsteed's unwillingness that Newton should com-
 municate the lunar observations to any other person: since we find a former hint to that effect in a
 note of Flamsteed on Newton's letter of February 16, 1694-5, page 152.

“ am setting on that work that was interrupted by them in the spring. My
“ exercise will devour no small part of my time, and therefore I shall desire
“ my friends to excuse me if I answer not their letters so fully nor readily as
“ formerly. *However, when you want more of my lunar observations, I will*
“ *cause them to be transcribed, and it will be no trouble.*” Whether the
remainder of these observations were forwarded to Newton, we have no
means of judging: but we find that, as late as the year 1700, Flamsteed was
willing to impart *more* lunar observations if required. See page 175. The
reader however will bear in mind, that six months after the date of this last
letter, Newton was appointed to the office of Warden of the Mint; and probably
had not then so much leisure time on his hands for such profound investi-
gations: which is indeed confirmed by himself in that extraordinary letter of
January 6, 1698-9, which he wrote to Flamsteed. See page 166.

I leave the public now to decide whether there is the least particle of evidence
to show that Flamsteed withheld, or manifested any disposition to withhold his
lunar observations from Newton: whether, on the contrary, Flamsteed did not
exhibit an earnest desire to promote the great object that Newton had in view:
and whether, in fact, the perfecting of it was not in a great measure owing to
Flamsteed's voluntary services. We have seen that he had anticipated New-
ton's wishes, by forwarding a list of all his lunar observations, for an appro-
priate selection: that he had transmitted *all* those which had been asked for:
that he had, in fact, exhausted the *whole* of his *mural arc* observations which
were of the most importance, and had entered upon his *sextant* observations:
moreover that he had sent not only the computed places of the moon, but also the
elements of those computations. It should be remembered likewise that, during
this interchange of letters with Newton, Flamsteed was in constant correspond-
ence with *numerous other individuals*, to whom he made frequent communi-
cations of his observations; as I shall more particularly mention in the sequel.
That disputes *subsequently* arose between these distinguished individuals, and
that those disputes gave rise to sad exhibitions of feeling and temper is greatly
to be deplored. Of the *origin* of those disputes we are but imperfectly informed

by any documents that I have seen: but I apprehend that they may be traced to Flamsteed's opinion that he was not fairly treated by Newton in his not having kept his promise relative to the communication of the lunar theory: since the quarrel does not appear to have commenced till about that period; and we know that it was a constant source of complaint with Flamsteed. Another reason may be that which is stated by Flamsteed himself, page 63, who says, "He was not so friendly as formerly because I could not confirm Mr. Halley's and Dr. Gregory's assertions concerning his corrections of the Horroxian lunar theory." It was also probably heightened by Newton's evident dislike, at least in later years, (a feeling which does not appear to be satisfactorily accounted for, especially after his repeated and positive assurances to the contrary, see page 151,) to acknowledge publicly, as he had often acknowledged privately, the assistance which he had derived from the observations made at the Royal Observatory; or even to permit Flamsteed to publish any allusion thereto. Like most disputes however of this kind, the cause was perhaps at first trifling, and might soon have died away, had not Flamsteed been afterwards brought into collision with Newton on the subject of printing the Greenwich Observations: for in the interval we find Flamsteed, in his letter to Sir Christopher Wren, page 173, alluding to Newton in terms of respect as one of his *friends*: and in his letter to Mr. Lowthorp, page 175, he exhibits a manifest bias in Newton's favour, notwithstanding that letter was evidently written under a state of excitement. It appears also that they occasionally dined at each other's houses: see pages 73, 231, and 232. But the mode and process of printing the Greenwich Observations opened a fruitful source of discord. Flamsteed did not augur well of it, from the manner in which it was commenced; and with such a presentiment the business was not likely to go on very cordially: in fact, in a very short time (as Mr. Whewell correctly expresses it) "the whole of the proceedings became a train of anger, harshness, confusion, and delay."

The transactions however of that unfortunate period should be carefully separated from the preceding portion of Flamsteed's history; since we shall

presently see that the subsequent dispute, here alluded to, has nothing to do with the question whether Flamsteed was or was not aware of any existing crisis in the history of astronomy ; or whether he perfectly understood or was thoroughly ignorant of Newton's new theory ; or whether he did or did not furnish Newton with his lunar observations for the purpose of perfecting that theory ; or whether he was or was not convinced that such observations were necessary. The publication of the Greenwich Observations, like similar publications of the present day, was required not for a *special*, but for a *general* purpose ; to place on record the state of the heavens at that period, for the benefit of the existing generation and posterity. Indeed they were never expressly applied, after their publication, either by Newton or any of his contemporaries, to the verification of the lunar theory ; at least, we have no recorded evidence of the fact : neither could they be so applied until tables had been constructed agreeably to Newton's theory, and published for general use. Newton had, in fact, previously gleaned from them all that he wanted. Even on the publication of Halley's tables, which did not take place till 40 years afterwards, the comparisons of the true and computed places of the moon were all taken from his own observations, and not one of them from Flamsteed's. It is proper to keep these points in view, to enable us to form a correct opinion on the conduct of the respective parties during the progress of those unhappy proceedings which *subsequently* occurred. This brings me to the *third* point of inquiry, relative to Flamsteed's supposed objection to the printing of his astronomical observations.

For the purpose of elucidating this matter, it will be requisite to take a slight review of its history. Flamsteed had scarcely been settled two years in the observatory, when it appears that the scientific public were anxious to learn the results of his observations. In a letter to Sir Jonas Moore, dated July 16, 1678, in page 116, he has given such valid reasons why it would be improper to publish anything at that early period, that I am unable to add a single argument to what he has there stated. Again, about two years after the erection

of the mural arc, we find Newton also suggesting to him the propriety of publishing a catalogue of the principal stars: but here likewise I must refer again to Flamsteed's "substantial reasons" for not complying with that wish, expressed in his letter of February 24, 1691-2 (page 129); a letter which, by a strange confusion of dates and circumstances, has been brought forwards, in the disputes of 1711, as expressive of Flamsteed's opinion on the subject, *twenty years afterwards*. Again, in another letter which he wrote at a still later date to Dr. Smith, on Oct. 26, 1700, preserved in the Bodleian library at Oxford (with a copy of which, and of Dr. Smith's answer thereto, I have been favoured by Professor Rigaud, both of which are inserted at full length, in the Addenda at the end of this Supplement), he has entered into a very full and lengthened statement of his reasons for even then not going to press immediately; which, I trust, must be satisfactory to every unprejudiced reader. Flamsteed evidently knew what he was about in conducting the great work placed under his superintendence: he literally "walked in an untrodden path," since he had nearly everything to begin anew, and almost every element to determine afresh. He however pursued his work methodically, and was not to be put aside by the clamours of those who were impatient to see the result of his labours. He set about a regular and minute survey of the heavens; and, when he considered he had got a sufficient stock of observations, he entered upon the reduction of the stars in the order of the constellations. "Were it understood," (says "Flamsteed, page 246") "what time and pains are required to gain the observations necessary for a catalogue of the fixed stars, the trouble of calculations afterwards, and the vast labour in them for deducing their places from the observations, and dressing them up in a frame fit for the view of the public, they would not betray their own ignorance by asking why I do not print: but rather inquire how far I had gone in the work, and take care, if they know the importance of it, to *procure me help to finish it*. They may as well ask why St. Paul's is not finished." Again, he says, page 54, "Some people to make me uneasy, others out of a sincere desire to see the happy progress of my studies, not understanding amid what hard circumstances I lived, called hard upon me to print my observations."

The *call*, it is true, was easy enough, but *how* was that call to be answered? and *who* was to supply the *funds* for carrying it into effect? It is very easy for us, at the present day, to contemplate and approve the wishes of astronomers at that period, without at all considering how those wishes and demands were to be satisfied. It could scarcely be expected that Flamsteed should undertake also that risk and expense, in addition to those heavy charges with which he had already been burdened, and amidst "the hard circumstances in which he lived." Who, it may be fairly asked, would come forward at the present day to reduce and print the Greenwich observations, or those made at Edinburgh, Paramatta, and the Cape of Good Hope, if the Government were now to withdraw their support to those publications? Who would have printed the Cambridge observations, if the University had not nobly stepped forward, and thus exhibited the first specimen of the proper mode in which astronomical observations should be presented to the world? What individual of the present day will take upon himself to print the observations made at Oxford or Dublin, which remain on the shelves of the observatory unnoticed and almost unknown? These are questions, which there can be no difficulty in solving: and the answers are equally applicable to Flamsteed's times as to the present*.

Flamsteed however looked forward to a more appropriate period, when he could, with more confidence, appeal to the public. He pursued his work steadily, without regarding the vain and idle clamors that were made: and, his review of the heavens being nearly completed, he proceeded to reduce his observations. This reduction was not begun till the month of September, 1695 (see page 381); and although the major part of the constellations were finished before the end of 1701 (see page 67), yet the whole catalogue could not be considered as complete till some time after this period. It was then, and then

* Delambre very properly remarks, "Ce n'est pas le tout que de fonder un observatoire, et de doter l'astronome; il faudrait faire un fonds annuel pour l'impression, et imposer à l'astronome l'obligation de n'être jamais en retard d'une année." — *Hist. de l'Astron. au xviii. siècle*, page 115.

only, that Flamsteed seriously contemplated the publication of his observations*.

But, during this interval, he had by no means been sparing of his communications: for he kept up a very large correspondence with a great number of astronomers both at home and abroad, to whom he transmitted, from time to time, and with the greatest readiness, various observations that were requested of him; as also catalogues of such of the principal stars as he considered sufficiently well reduced, and (to those who were engaged in such special inquiries) copies of his solar, lunar, and planetary tables, besides explanations and discussions on various points of astronomy. This is sufficiently evident from that portion of his correspondence which is printed in the preceding part of the present work, where we find the names of Newton, Moore, Collins, Wallis, Caswell, Sharp, Bossley, Leigh, and others: but it is more fully confirmed by the *unpublished* portion of his correspondence, I mean those letters which still exist in manuscript at the Royal Observatory, and which fill three folio volumes, besides other detached proofs. Amongst that correspondence we find not only further letters from many of the parties just mentioned, but also from Hevelius, Cassini, Leibnitz, Römer, Zimmerman, Kirch, and others abroad, as well as from Derham, Pound, Molyneux, Witty, Stanyan, Gray, Wright, (who styles himself an "unworthy priest of the church of England"), Thomas, Greenbury, Young, Brattle, and others of our own countrymen, who (although perhaps for the major part unknown at the present day) were then engaged in astronomical inquiries of various kinds†. Indeed his correspondence alone seems to

* In his letter to Mr. Sharp, February 9, 1702-3, Flamsteed says, "I desire to have them [i. e. the observations, &c.] published as soon as may be; and, in order to it, that you will speak freely of them; * * * 'tis but just the public should defray all this charge; yet I ask no more than to hire help to copy my papers and books for the press, and to finish some necessary calculations and tables; and to print them at *my own* charge." For Flamsteed had made up his mind to print his observations prior to the offer of Prince George to defray the expence.

† Mr. Thomas Brattle of Boston in New England is the anonymous person alluded to by Newton in the *Principia* as having made such good observations of the comet of 1680: but he says, in his letter to Flamsteed, that he took no great pains on the subject.

Mr. Edward Greenbury of Beverley in Yorkshire was engaged on the lunar tables; and Flam-

have been almost sufficient for the employment and duty of a clerk; setting aside the numerous entries he had to make in his books, and the system of transcripts and re-arrangements of the same which he thought it right to adopt: of the extent of which no person can form an adequate idea without an actual inspection of the manuscripts.

But, to return to the printing of the observations. When Flamsteed had got his work in that state of forwardness that it was nearly fit to meet the public eye, he drew up an estimate of the number of pages which it was likely to occupy: which estimate was accidentally read at the Royal Society on November 15, 1704 (see page 75); and in consequence of what there passed, the expense of printing the observations was subsequently undertaken by Prince George of Denmark. It is needless for me to repeat here what has been so fully narrated in the preceding part of this work, relative to the progress of the printing; nor need I again allude to the delay, or the bickerings that took place whilst the press was employed on the *first* volume of the *Historia Cœlestis*. Those squabbles were probably not more remarkable than similar occurrences of the present day; and such, unfortunately, as too frequently take place where various individuals with opposite views and interests, and not very cordially inclined towards each other, are blended together, and striving to drive what is called a "hard bargain*:" yet perhaps not so great nor so inveterate, but

steed had, from time to time, sent him a great number of lunar observations. In a letter written to him March 21, 1697-8, Flamsteed says, "I am glad to find that you have begun with the Horroxian theory of the moon: 'tis the only truth as far as it goes, and Mr. Newton only adds some small inequalities to it, which he finds necessary by comparing the observations with the places calculated, and natural causes. The eccentricities are commonly too little; as also the equations of the apogee." In the same letter Flamsteed offers to send him copies of his solar and lunar tables, and such observations as he may want.

Mr. George Young of Bere Regis was occupied on rectifying the motions of Mars. In a letter which Flamsteed addressed to him on May 10, 1709, he says, "Please to let me know if you design a correction of the motion of Mars: by your demands you seem to have some such thing in your mind; if you have, you ought to begin with the Sun. Deal always plainly and sincerely with me: my solar tables shall be at your service, as well as they have been at other friends'; and although I had rather you should employ yourself upon Jupiter, yet for Mars you may assure yourself of my assistance, God sparing me life and health."

* Mr. Churchill, the publisher, was looked upon by Flamsteed as a very unnecessary personage

that they might, notwithstanding their provoking tendency, soon have been forgotten and forgiven, had they not terminated in a more open rupture, and thus led to matters of more importance and interest.

The *first* volume however, containing the *sextant* observations, was at length finished (December 1707), notwithstanding all these impediments; in which it is not always easy to distinguish, nor indeed is it now of much consequence to know, which party was in the right. Preparations were next made for printing the *second* volume, by far the most important of the two, since it was to contain the observations made with the *mural arc*: and it is to the publication of *this* portion of the work and the delays attending it, that I would wish to draw the reader's particular attention, as being that which is now said to have been more especially required for the confirmation of Newton's theory, and the publication of which it was therefore so desirable to expedite and secure. Flamsteed furnished the materials for this volume, by depositing in the hands of the Referees on March 20, 1707-8, the *whole* of the observations made with the mural arc from the date of its commencement to the end of December 1705, fairly copied out on 175 sheets of large paper, which had been ready above a year and a half (see page 261); and shortly after amended the catalogue which had been lodged in their hands as a pledge*. A little more skirmishing then took place between the "hard bargain" drivers; but before the printing commenced, Prince George died (October 28, 1708), and the whole of the proceedings were *suspended for three years*: the 175 sheets of manuscript, together with the catalogue (the materials in fact for that

in these proceedings; and one whom he considered as improperly running away with those profits on the sale of the work, to which he thought that he himself was more justly entitled. It appears also that some other *drones* were to be benefited at Flamsteed's expense. See pages 238, 248, and 255.

* Some persons have doubted whether this *amended* catalogue was *sealed up again*, when returned to Newton: but I apprehend that Flamsteed's direct assertion of the fact, in the presence of Newton, and Newton's reply thereto that "Dr. Arbuthnott had procured the Queen's order for *opening it*," (see page 294) is sufficient evidence on this point. It is also again mentioned by Flamsteed in another letter to Mr. Sharp, in page 298; and in a subsequent letter in page 317; as well as in a formal letter to Newton himself, in page 322. All these passages must refer to the *re-delivery* of the catalogue into Newton's hands, else they have no efficient meaning.

second volume) *remaining during the whole of that time in the hands of the Referees**. At the end of that period, namely in March 1710-11, Flamsteed learnt for the first time (no communication having been had with him on the subject during the interval) that his packet containing the catalogue had been broken open, and that not only the catalogue itself was at press, but also that the observations (copied out on the 175 sheets of paper as above mentioned) were likewise in the course of being printed in a garbled and mutilated state †.

Flamsteed was, of course, very much annoyed and irritated at this unexpected piece of intelligence: he saw, at once, that his favourite plan of printing his observations in detail, in the order in which they were made, and the only way indeed in which they could be essentially useful to the future astronomer, was without his knowledge or consent about to be sacrificed to a scheme that would render them of little or no practical utility, and compromise his own character as an observer. He likewise found that the places of the moon, which he had from time to time communicated to Newton, with an express understanding that they were not to be published because they were deduced from an imperfect catalogue of the stars, were annexed to the work. He was convinced that this scheme had been long in agitation, since it must have taken the Referees a considerable time to dissect and arrange the observations in the manner in which they were then prepared and sent to the press ‡. Upon what grounds this clandestine and improper conduct can be justified I have ever been at a loss to imagine: and I have always regretted (in common, I am sure, with

* It was declared by Flamsteed and fully understood by the Referees that the catalogue would still want *further* correction, before it could with propriety be printed.

† No demand was ever made by the Referees for any observations subsequent to the year 1705: indeed it seems by Dr. Arbuthnott's letter of March 26, 1711, in page 281, that their power did not extend beyond that date.

‡ The same remark may perhaps be applied to the *catalogue*: and therefore Flamsteed's assertion that the Queen's order (if obtained at all) had been obtained *after* the offence was committed, is probably correct; as that order would not have been given prior to February, and the catalogue, containing the additional stars by Halley, was at press in the following month, and actually finished by the month of June.

every other reader) to find Newton's name mixed up with a transaction of this kind; since it is, in my opinion, the only portion of the series of disputes recorded in this volume, that is worthy of a serious refutation: all the other sudden ebullitions of temper and apparent perversity of conduct being mere venial offences of our common nature. But I suspect that it was in that day, as at the present hour, that individuals of high and honourable character (when acting in concert with others having interested objects in view, and not quite so scrupulously austere in their conduct as themselves,) may oftentimes be led to countenance and sanction certain acts, which as private persons, and on their sole responsibility, they would cautiously avoid. I have more than once had occasion to call the public attention to this singular anomaly in the ordinary intercourse of human affairs*.

Flamsteed attempted to counteract this scheme, by printing his original observations at his own expense. With this view he endeavoured to recover the 175 MS. sheets out of Newton's hands; and on his refusal commenced legal proceedings against him. It is needless to repeat this unpleasant tale, which has been already described in the preceding history; it will be sufficient to state that Flamsteed was eventually obliged to copy out these manuscripts again for the press, at a considerable expense: and it is to him alone, and not to the Referees, that we are indebted for the volume of mural arc observations which we now possess†.

I imagine therefore that it may now be left to the candid and unbiassed judgment of the public to decide whether there is the slightest foundation for the opinion that Flamsteed opposed any impediment to the publication of his

* See my *Doctrine of Life Annuities and Assurances*. Vol. ii., page 507.

† Delambre, alluding to this subject, has the following remark: "Il importe d'avoir la situation et l'état du ciel à toutes les époques. Des annales non interrompues de l'astronomie seraient parfois très utiles à consulter. Des observations, même médiocres, acquièrent du prix avec le temps, quand les erreurs qu'on peut y soupçonner, deviennent insensibles, parce qu'elles sont réparties sur un plus grand intervalle: mais il importe qu'elles soient données avec tous les détails qui sont nécessaires au calculateur, qui veut en entreprendre de nouveau la réduction sur de meilleurs élémens. C'est l'avantage qu'on trouve dans l'*Histoire Céleste de Flamsteed*."—*Hist. de l'Astron. au xviii. siècle*, page 114.

astronomical observations; and least of all to this latter portion of them, which was by far the most important and valuable: or whether, on the other hand, Newton exhibited any great anxiety for their speedy appearance, in order to complete his theory of gravitation. For, it is seen that Flamsteed had, as far back as the year 1708, and within a few months after the close of the printing of the *first* volume, placed in the hands of the Referees the *whole* of the observations that were then intended to form the *second* volume of the work, fairly copied out on 175 sheets of paper for the press: and that those Referees, *after keeping them dormant for three years*, at last made them over (as we shall presently see) to another body of superintendents, who sent them to the press without Flamsteed's knowledge or concurrence, and in a manner totally unfit for the purposes which he intended.

It should be remarked that the duty and office of the Referees, with whom the above-mentioned documents were originally deposited, expired on the decease of the Prince; and we find, in page 275, that they had surrendered up their trust. It therefore still remains to be shown by what authority the *new* body of superintendents* could claim the right of breaking open the packet containing the catalogue, which Flamsteed had deposited with other parties, which he had always considered as *his own* property, and which he says (page 94) he never designed or intended that Newton, or any but himself, should publish without his concurrence. Of the motives which induced Newton to call for such a deposit, in the first instance, and of the special purposes for which it was thus so guardedly placed in his hands, I am unable to produce any information in addition to that which has been already laid before the public. Flamsteed says (page 295) that it was "put into their hands as a pledge for securing the delivery of the remaining part of the observations, then not ready †." But this would not apply to the re-deposit of it in its amended

* See the note in page 93.

† The "remaining part" of the observations, here mentioned, must allude to those intended to form the second volume of the *Historia Cælestis*: since it appears that Flamsteed had delivered "the copy of the first volume of observations *at once* to the referees." See his letters to Mr. Sharp, of November 28, and December 12, 1705, in pages 256 and 257. Moreover, Newton had pos-

state, in the year 1708, because Flamsteed had actually placed in the hands of the referees the *whole* of the copy of the second volume, *prior* to the re-delivery of such amended catalogue. There is one argument however that has been adduced in justification of the alleged breach of faith, in breaking open the packet, which in my opinion is not borne out by fact; and therefore I shall take leave to enlarge somewhat on the subject. This brings me to the fourth and last point of inquiry, mentioned in page 677.

It has been considered by some that this new body of trustees were justified in taking this step on the plea that the contents were *public* property. I shall not stop to inquire whether, even on this consideration, they were warranted in thus setting aside, at one blow, and with the strong arm of power, without any communication of their wishes or intention to Flamsteed, all the *mutual* conditions under the faith of which, either expressed or implied, the deposit was placed not in their hands, but in the hands of an extinct body; but shall come at once to the question at issue, namely whether the catalogue contained in the packet was, or was not, *public* property. Now if I show (as I think I am enabled to do) that the *original observations* were not the property of the public, it will follow *à fortiori* that they could not have the slightest claim to the *catalogue*, which had been deduced from those observations at Flamsteed's sole expense and labour. This question was brought to issue on the death of Dr. Bradley, whose executors (following the example of the executors of Flamsteed and Halley*) removed from the Royal Observatory the *whole* of his books and papers, amongst which were the original entries of all the astronomical observations made by him as Astronomer Royal; and retained them session of the original books of entries long before the first volume of the work was sent to the press: and detained two of them, against Flamsteed's remonstrances, for several years after the work was finished. See page lx.

* Halley's original manuscripts were *purchased*, after his decease, by the Government, in a manner similar to that in which we have seen that Flamsteed's were obtained. They are now in the library of the Royal Observatory at Greenwich; and a MS. attested copy of them is deposited in the library of the Royal Astronomical Society.

as the absolute property of the deceased, to be accounted for in the legal distribution of his assets. The Government resolved to try the question of right, and accordingly filed a bill in the Court of Exchequer in order to bring the matter to issue. The progress and issue of this cause will be best seen from the following statement extracted from the Preface to *Bradley's Observations*.

“ Upon the author's decease in 1762, his executors and representatives, who
 “ thought themselves legally entitled to dispose of these valuable remains, were
 “ fully minded (both from motives of good will to mankind and out of respect
 “ to the memory of their ingenious and learned friend and relation) to make
 “ them public as soon as possible, and were actually concerting measures for
 “ that purpose, when they were surprised by a demand of the observations on
 “ the part of the Royal Society, who pretended to a right of property in them*.
 “ This claim however was soon given up as groundless; but was presently suc-
 “ ceeded by a similar one on the part of the Crown, made at the instance of
 “ the Commissioners of the Board of Longitude. The merits of this latter
 “ claim were founded on the Doctor's having been appointed to the office of
 “ Astronomical Observator at the Observatory at Greenwich, with a salary of
 “ £100 a year, and directed to apply himself with the utmost care and diligence
 “ to the rectifying the tables of the motions of the heavenly bodies and places
 “ of the fixed stars, in order to find out the so much desired longitude at sea.
 “ That the Doctor did so apply himself as was required of him, and that with
 “ consummate skill and attention till the time of his death, the observations
 “ made by him do abundantly testify. But that the property of these invaluable
 “ labours should have departed from him in consideration of the small and inade-
 “ quate salary he had received†, or of a pension afterwards granted him by
 “ His Majesty King George II. in 1751 (not as an increase of salary, but as a
 “ gratuitous acknowledgment of his personal merit, and of certain services
 “ performed by him independent of his office), was what his executors could
 “ not conceive to be consonant with reason and equity, as it was not with the

* Probably as Visitors of the Royal Observatory.—F.B.

† “ Not exceeding £90 a year.”

“ practice and usage which had been before observed upon the decease of his
 “ predecessors in office *. It is not however the business or design of the
 “ editor to discuss the merits of this question, but simply to state that it fur-
 “ nished matter for a lawsuit, which was commenced on the part of the Crown
 “ in 1767, and continued till the year 1776, when it was abandoned : and
 “ the right of the observations thereby admitted to rest in those who were
 “ legally entitled to them by the author’s will.” I shall merely add, by way of
 supplement to this statement, and for the information of the reader, that Mr.
 Yorke and Mr. De Grey, the Attorney and Solicitor General at that time,
 differed in opinion as to the question of right, and that it was not till Mr. Dun-
 ning was made Solicitor General, who concurred in opinion with Mr. Yorke,
 that the proceedings were actively commenced.

There is this striking difference however between the cases of Flamsteed and
 Bradley. The instruments, with which the observations of Flamsteed were
 made, were his own property : the very books in which those observations were
 entered, the pens and the ink with which they were written, the paper on which
 they were copied, were all furnished at his own cost, and not at the expense of
 the public, who contributed nothing but the paltry salary of £100 per annum for
 “ labour harder than *thrashing* †.” But Bradley, as is well known, was amply
 supplied with a variety of instruments of the best kind at the public expense ;
 and had moreover an addition of £250 per annum to his salary. If therefore the
 Government could not substantiate their right to the original manuscript of the
 observations of Bradley, still less were they warranted in asserting a claim,
 either in law or equity, to those of Flamsteed ‡. And if not to the *observations*,

* “ These were Dr. Flamsteed and Dr. Halley ; of whom the former printed a considerable part
 “ of his observations in his lifetime, for his own emolument ; and the daughter of the latter received
 “ a compensation, after her father’s death, for those which he left behind him.”

† It appears, from the note in page 732, that there was a deduction of £10 from this salary.
 This is confirmed by a letter from Flamsteed to the Board of Ordnance (still existing amongst
 Flamsteed’s MSS.), wherein he applies, but in vain, to have this tax removed.

‡ Flamsteed himself was evidently of this opinion ; for, when he found that Newton would not
 return the 175 sheets of manuscripts, he commenced legal proceedings against him, for their reco-
 very : see page 322. Mr. Caswell also took the same view of the case : see page 240. It should

they could have less pretension to the *catalogue* of stars, which together with their corresponding *longitudes and latitudes*, and their *precessions*, had been deduced by Flamsteed himself, at a very considerable expense of time, and labour, and money, to the perfecting of which the whole of a very long life had been devoted, which indeed he considered as “the glory of the nation,” to which the Government had not expressly contributed one farthing, and to which they consequently had not the shadow of a claim. Flamsteed, in fact, considered that this catalogue was as much his own *private* property as the clothes upon his back: and to *rob* him of the one (as he emphatically terms it) was doubtless considered in his eyes as gross a violation of justice, as to strip him of the other. It was under a feeling of this kind that he retorted upon Newton in the lamentable scene that took place between them at the apartments of the Royal Society, about six months afterwards. (See pages 228 and 294.)

But, I shall here close this unpleasant and ungrateful subject; to the reconsideration and revision of which I have been led solely by a desire to place Flamsteed's character and conduct in their true and simple colours, disentangled from all those extraneous matters with which they have been inadvertently involved. In the execution of this task I trust that nothing which I have advanced will be construed as expressive of the slightest inclination to say aught, beyond the strict line of historical truth, against the memories of those who have been occasionally brought forward on the stage; and whose splendid talents, and important services to science, (whatever failings which they may have possessed as the common lot of humanity,) will ever be justly valued, and borne in pleasing remembrance by a grateful posterity.

be recollected that at this period the law of *copyright* had not been established, which made Flamsteed the more cautious in his communications. See pages 300, 310, and 313.

[Extract from Dr. Gregory's *Astronomiæ Elementa*, page 332.]

SCHOLIUM.

Libet lunæ theoriam à D. Newtono in praxi usurpatam hic adjungere, quâ difficillimum istud et ab astronomis hactenus desperatum consecutus est celeberrimus philosophus; nimirum ut lunæ locum, etiam extra syzygias, et in ipsis cum sole quadraturis, cœlo adeo consentientem ex calculo definiat, (sicuti per plurima lunæ loca à Cl. D. Flamstedio observata expertus est,) ut dissensus à cœlo, etiam cum maximus, vix duo scrupula prima adæquet; plerumque adeo sit exiguus, ut observandi incertitudini sit jure imputandus. In hac calculi forma, quam ipsis auctoris verbis expressam astronomis sistimus, non attingit omnes omnino inæqualitates quarum causæ superius sunt explicatæ, nedum illas de quibus adhuc tantum est suspicio; sed omissis iis quas se invicem compensaturas novit, aliisque minoris efficaciz, eas duntaxat æquationibus et tabulis coercet, quarum majores sunt vires et effectus sensibiliores*.

Lunæ Theoria Newtoniana.

“Observatorium Grenovicense occidentalius est Parisiensi 2^{gr}. 19', Uraniburgo 12^{gr}. 51'. 30'', et Gedano 18^{gr}. 48'.

“Solis et lunæ motus medios ab æquinotio verno in meridiano Grenovicensi pono sequentes: nempe anno 1680 Decembris die ultimo stylo Juliano

* This introductory paragraph was written by Gregory; the subsequent part, which was furnished by Newton, being printed within inverted commas. It is the more necessary to attend to this remark, as the assertion which I have made in the note in page 370, that “in the *Theoria Lunæ* there is not a single allusion made to Flamsteed,” has been called in question, under an erroneous impression that Newton's *Theory of the Moon* has not been published, under that title, except in Gregory's *Astronomiæ Elementa*, where it is said the acknowledgment will be found. I first saw the *Theoria Lunæ*, however, unaccompanied by any introduction, in Horsley's edition of Newton's Works, vol. 3, page 245: and on comparing it with the copy in Gregory's Elements, I find them to be precisely alike. Flamsteed's name is certainly not mentioned in it, nor is any allusion made to him; as the reader may plainly see, by the copy here reprinted. It is true that Gregory has, in the above introductory paragraph, incidentally noticed the labours of Flamsteed in furnishing materials for this theory; but Newton himself is wholly silent on the subject.

“ meridiæ, motus medius solis est $9^{\text{sig}}.20^{\text{gr}}.34'.46''$. Apogæi solis $3^{\text{sig}}.7^{\text{gr}}.23'$.
 “ $30''$. Motus medius lunæ $6^{\text{sig}}.1^{\text{gr}}.35'.45''$. Apogæi lunæ $8^{\text{sig}}.4^{\text{gr}}.28'.5''$.
 “ Nodi ascendentis orbitæ lunaris $5^{\text{sig}}.24^{\text{gr}}.14'.35''$. Et anno 1700 Decem-
 “ bris die ultimo stylo Juliano meridiæ, motus medius solis est $9^{\text{sig}}.20^{\text{gr}}$.
 “ $43'.50''$. Apogæi solis $3^{\text{sig}}.7^{\text{gr}}.44'.30''$. Motus medius lunæ $10^{\text{sig}}.15^{\text{gr}}.19'$.
 “ $50''$. Apogæi lunæ $11^{\text{sig}}.8^{\text{gr}}.18'.20''$. Et Nodi ascendentis $4^{\text{sig}}.27^{\text{gr}}.24'.20''$.
 “ Viginti enim annis Julianis, sive diebus 7305, solis motus est $20^{\text{rev}}.0^{\text{sig}}.0^{\text{gr}}$.
 “ $9'.4''$. Motus apogæi solis $21'.00''$. Lunæ motus est $267^{\text{rev}}.4^{\text{sig}}.13^{\text{gr}}.34'$.
 “ $5''^*$. Apogæi lunaris motus $2^{\text{rev}}.3^{\text{sig}}.3^{\text{gr}}.50'.15''$. Nodi motus $1^{\text{rev}}.0^{\text{sig}}.26^{\text{gr}}$.
 “ $50'.15''$. Omnes prædicti motus sunt à puncto æquinocitii verni. Quod si
 “ ab illis subducatur ipsius æquinocitialis puncti motus in antecedentia interea
 “ factus, sc. $16' 40''$; manebunt motus respectu fixarum in annis 20 Julianis
 “ nempe motus solis $19^{\text{rev}}.11^{\text{sig}}.29^{\text{gr}}.52'.24''$. Apogæi solis $4'.20''$. Lunæ
 “ $267^{\text{rev}}.4^{\text{sig}}.13^{\text{gr}}.17'.25''^*$. Apogæi lunæ $2^{\text{rev}}.3^{\text{sig}}.3^{\text{gr}}.33'.35''$. Nodi lunæ
 “ $1^{\text{rev}}.0^{\text{sig}}.27^{\text{gr}}.6'.55''$.

“ Secundum hunc computum annus tropicus est $365^{\text{dier}}.5^{\text{hor}}.48'.57''$. Annus-
 “ que sydereus $365^{\text{dier}}.6^{\text{hor}}.9'.14\frac{1}{2}''$.

“ Motus medii luminarium suprapositi variis afficiuntur inæqualitatibus.

“ Et primo sunt annuæ æquationes dictorum motuum mediorum solis et lunæ,
 “ et apogæi nodique lunæ. Æquatio annua motus medii solis pendet ab
 “ excentricitate orbitæ telluris circa solem, quæ est partium $16\frac{1}{12}$, qualium
 “ mediocris distantia solis à terra est 1000: indeque vocatur æquatio centri:
 “ estque, cum maxima, $1^{\text{gr}}.56'.20''$. Maxima æquatio annua motus medii
 “ lunæ est $11'.49''$. Apogæi ejus $20'$. Nodique $9'.30''^\dagger$. Atque quatuor
 “ istæ æquationes annuæ sunt semper sibi mutuo proportionales. Ideoque cum

* Erroneously printed 247^{rev} in Dr. Gregory's book.

† Delambre says (*Hist. de l'Astron. au xviii. siècle*, page 29), that these equations of the apogee and node are certainly due to theory: and yet it would appear (see the note in page 692), that Flamsteed had previously inserted similar equations in his tables. The numbers here given are slightly altered in the second edition of the *Principia*: and Newton has also there shown that they vary according to the distance of the sun from the earth. In the third edition, he says that the mean motions of the apogee are not yet sufficiently ascertained. Flamsteed seems to have doubts of the same kind: see page 698.

“ earum aliqua est maxima, tres reliquæ sunt etiam maximæ; diminutâ vero
 “ quâvis, minuuntur etiam et reliquæ in eâdem ratione: unde datâ æquatione
 “ annuâ centri solis, dantur et tres reliquæ annuæ æquationes congruæ; illius
 “ igitur tabula sufficit: nam si æquatio annua centri solis tempori cuivis con-
 “ grua inde deprompta vocetur P, et fiat $\frac{1}{10} P = Q$, $Q + \frac{1}{60} Q = R$, $\frac{1}{6} P = D$, $D + \frac{1}{30} D = E$,
 “ et $D - \frac{1}{60} D = 2F$; erit æquatio annua eidem tempori congrua lunæ quidem R,
 “ apogæi lunaris E, et nodi F. Adnotandum, si æquatio centri solis est
 “ addenda, æquationem lunæ prædictam esse subducendam, apogæi lunaris
 “ æquationem addendam, nodi vero æquationem subducendam: et è contra, si
 “ æquatio centri solis est subducenda, addenda erit lunæ æquatio, apogæi vero
 “ subducenda, et nodi addenda.

“ Alia est æquatio motûs medii lunæ, pendens à situ apogæi lunaris
 “ respectu solis, quæ maxima est cum apogæum lunæ versatur in octante cum
 “ sole, et nulla cum illud ad syzygias vel quadraturas pervenerit *. Æquatio
 “ hæc, quando maxima, ad $3'.56''$ ascendit, sole in perigæo versante: si vero
 “ sol apogæum teneat, non ultra $3'.34''$. In aliis distantiiis solis à terra
 “ æquatio hæc maxima est reciproce ut cubus istius distantie. At cum
 “ lunæ apogæum est extra octantes, æquatio dicta evadit minor, estque ad
 “ maximam, positâ eâdem distantiaâ terræ et solis, ut sinus duplæ distantiaâ
 “ lunaris apogæi à proxima syzygia vel quadratura ad radium. Additur hæc
 “ motui lunæ, dum lunæ apogæum transit à solis quadrato ad syzygiam; sed
 “ inde subducitur in transitu apogæi à syzygia ad quadratum.

“ Alia porro est motûs lunæ æquatio, pendens ab aspectu nodorum orbitæ
 “ lunaris cum sole; estque maxima cum nodi in solis octantibus versantur,
 “ evanescitque cum hi ad syzygias aut aspectum quadratum appellunt †. Æqua-
 “ tio hæc proportionalis est sinui duplæ distantie nodi à proxima syzygia aut
 “ quadratura, cumque maxima ad $47''$ ascendit. Additur hæc motui lunæ, dum

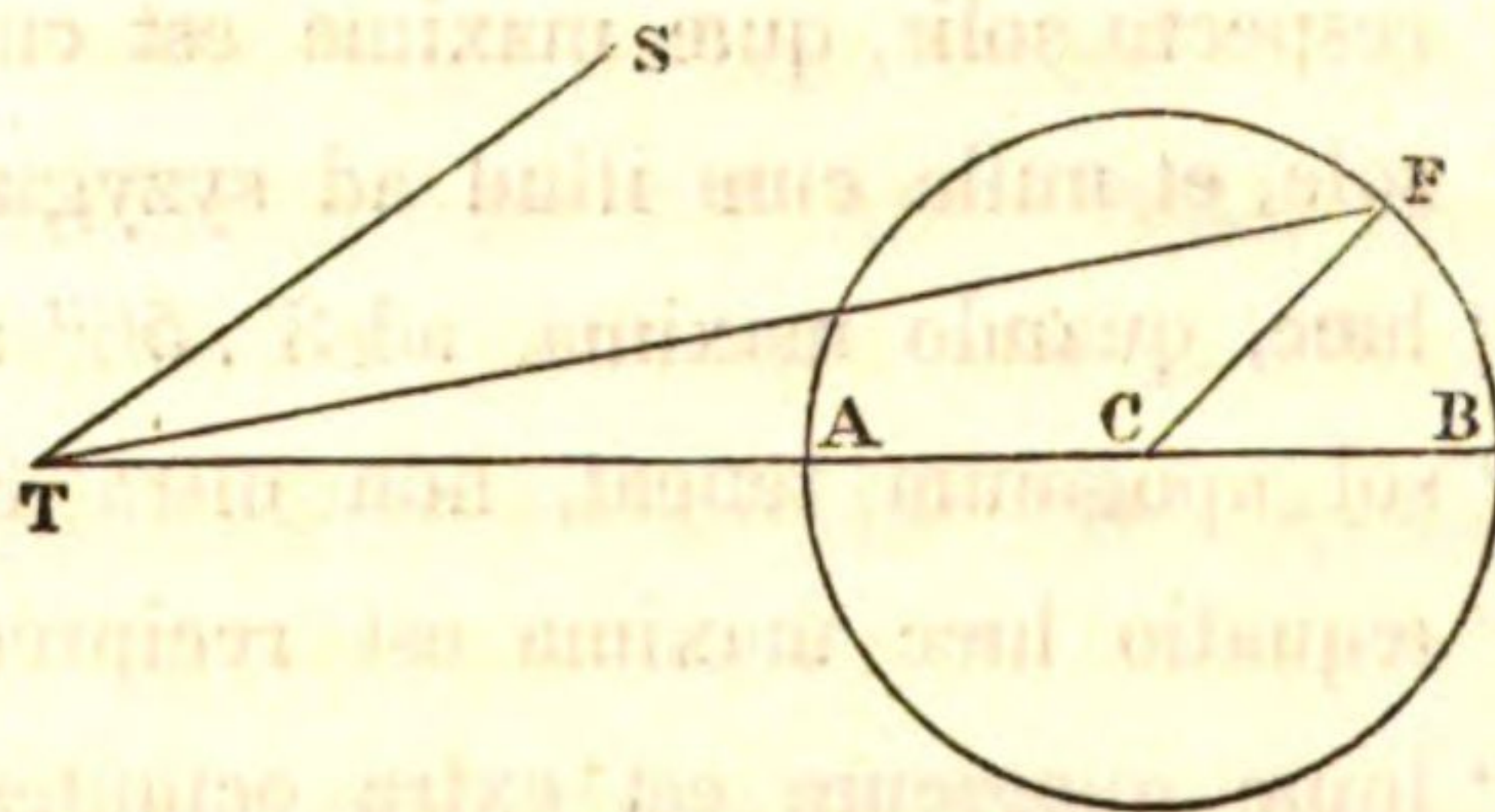
* This equation, depending on twice the annual argument, or $2(D - A)$ according to Delambre's system of notation, does not amount to so much as $1'$ in the tables of Mayer, Burgh, or Burckhardt. See the note in page 743.

† This equation, depending on $2(\odot - \Omega)$, is somewhat greater in the tables of Mayer, Burgh, and Burckhardt.

“ nodi transeunt à solis syzygiis ad ejusdem quadraturas; et subducitur in
 “ eorum transitu à quadraturis ad syzygias.

“ A solis loco vero aufer motum medium apogæi lunæ æquatum, ut supra
 “ est ostensum; residuum erit argumentum annuum dicti apogæi. Exinde
 “ computentur lunæ excentricitas et secunda æquatio ejus apogæi modo
 “ sequenti [*qui in aliis quibusvis intermediis æquationibus computandis locum*
 “ *etiam habet:*] Referat *T* terram; *TS* rectam conjungentem terram et solem;
 “ *TACB* rectam à terra ductam ad locum medium apogæi lunaris ut supra
 “ æquatum; angulus *STA* argumentum annuum dicti apogæi; *TA* lunaris
 “ orbitæ excentricitatem minimam; *TB* ejusdem excentricitatem maximam.
 “ Biseca *AB* in *C*, centro *C* per *A* describe circumulum *AFB*; fiat angulus *BCF*
 “ æqualis duplo argumento annuo: juncta recta *TF* erit lunaris orbitæ ex-
 “ centricitas, angulusque *BTf* erit se-
 “ cunda apogæi lunæ æquatio*.

Ad
 “ horum determinationem sit mediocris
 “ distantia lunæ à terra, sive orbitæ luna-
 “ ris semidiameter, partium 1000000:
 “ ejus maxima excentricitas *TB* erit par-
 “ tium 66782, et minima *TA* earundem 43319; adeo ut maxima orbis ejus
 “ æquatio, cum sc. apogæum est in syzygiis, sit $7^{\text{gr}}.39'.30''$, vel forsā $7^{\text{gr}}.40'$
 “ $00''$ (suspicio enim est hanc mutari pro situ apogæi in ϖ vel ϖ); cum vero
 “ illud in solis quadrato hæret, dicta maxima æquatio sit $4^{\text{gr}}.57'.56''$, et ut
 “ maxima apogæi æquatio sit $12^{\text{gr}}.15'.4''$.



“ Constructâ ex hisce principiis tabulâ æquationum apogæi lunæ et excen-
 “ tricitatum ejus orbitæ ad singulos gradus argumenti annui, unde excen-
 “ citas *TF* et angulus *BTf* (sc. æquatio secunda et præcipua apogæi) dato
 “ tempori congruentes facile possint depromi; ad locum apogæi lunæ primo
 “ æquatum ut supra addatur æquatio modo inventa, si argumentum annuum
 “ minus sit 90^{gr} , aut majus 180^{gr} , minus vero quam 270^{gr} ; secus vero ab eo

* This mode of explicating the rule for deducing the eccentricity of the lunar orbit, and the second equation of the apogee, is similar to that given by Horrox and Flamsteed. See page 692.

“ subducatur: summa vel differentia erit apogæi lunaris locus secundo æqua-
 “ tus; quo subducto ex lunæ loco tertio æquato, relinquitur lunæ anomalia
 “ media dato tempore congrua. Porro, ex hac anomalia lunæ media et
 “ modo inventa orbis excentricitate habebitur (ope tabulæ æquationum centri
 “ lunæ ad singulos anomalie mediæ gradus, et aliquot excentricitates v. g.
 “ 45000, 50000, 55000, 60000 et 65000 fabricatæ) prostaphæresis sive æquatio
 “ centri lunæ, ut vulgo*; quâ subductâ in priori anomalie mediæ semicirculo,
 “ additâ vero in posteriori ad locum lunæ hactenus ter æquatum, prodit lunæ
 “ locus quarto æquatus.

“ Maxima lunæ variatio, sc. quæ contingit cum luna est in octantibus solis,
 “ est fere reciproce ut cubus distantie solis à terra. Capiatur ea $37'.25''$
 “ cum sol est perigæo, et $33'.4''$ cum in apogæo: fiantque variationis hujus
 “ in octantibus differentie reciproce ut differentie cuborum distantiorum solis
 “ à terra, et exinde construatur tabula prædictæ variationis lunæ in octantibus
 “ solis (ejusve logarithmi) ad singulos denos vel senos vel quinos gradus ano-
 “ malie mediæ: et pro variatione extra octantes, fiat ut radius ad sinum duplæ
 “ distantie lunæ à proxima syzygia vel quadratura ita supra inventa variatio in
 “ octante ad variationem dato aspectui congruam, quæ addita loco lunæ supra
 “ invento in primo et tertio quadrante (computando à sole), aut ab eodem
 “ subducta in secundo et quarto, exhibet lunæ locum quinto æquatum †.

“ Rursus, ut radius ad sinum summæ distantiarum lunæ à sole et apogæi
 “ lunæ ab apogæo solis (vel sinum excessûs istius summæ supra 360^{gr}) ita

* Instead of constructing the tables of the equations of the centre for the several eccentricities here stated, Flamsteed originally preferred taking only the least, the mean, and the greatest. He afterwards, however, in his MS. tables, pursued a different and more convenient method, by computing the values for the greatest equations of the centre corresponding to 5, 6, and 7 degrees. See page 210, and the note in page 705. The whole is a mere matter of arrangement, since they all come to the same point.

† This rule for determining the Variation is omitted in the second edition of the *Principia*: and the reader must refer to Prop. 29 for the mode of determining its amount. Flamsteed, in his MS. tables, has computed the Variation for different maxima: but Halley has assumed $35'.10''$ for the maximum, and corrected the error, thence arising, by a small equation depending on the sun's anomaly.

“ $2'.10''$ ad sextam loci lunæ æquationem, subducendam si prædicta summa
 “ vel dictus excessus minor sit semicirculo, addendam si major *.

“ Fiat etiam ut radius ad sinum distantiae lunæ à sole ita $2'.20''$ ad æqua-
 “ tionem septimam. Hanc aufer quando lunæ lumen augetur, et (è contra)
 “ adde cum illud minuitur; et prodibit lunæ locus septimo æquatus, quique
 “ est locus ejus in propria orbita. Notandum æquationem, quæ hic effertur
 “ per mediocrem quantitatem $2'.20''$, non esse ejusdem semper magnitudinis,
 “ sed augeri et minui pro situ lunaris apogæi. Nam si apogæum lunare con-
 “ junctum fuerit cum solis apogæo, prædicta æquatio est circiter $54''$ major;
 “ sin illi oppositum, tantundem minor: libratque inter maximam quantitatem
 “ $3'.14''$. minimamque $1'.26''$ †. Atque hæc obtinent ubi apogæum lunare
 “ est in solis syzygiis; ubi vero illud in solis quadrato hæret, minuenda est
 “ æquatio prædicta 50 circiter scrupulis secundis aut integro scrupulo primo,
 “ quando apogæum lunæ et solis apogæum conjuncta sunt; si vero sunt oppo-
 “ sita, propter observationum penuriam affirmare nequeo augendane sit illa, an
 “ minuenda. Immo de suprapositis incremento et decremento æquationis
 “ $2'.20''$, propter observationum satis accuratarum defectum, certo statuere non
 “ ausim.

“ Si æquationes sexta et septima augeantur vel minuantur in ratione reci-
 “ proca distantiae lunæ à terra, hoc est, in directa ratione parallaxis horizontalis
 “ lunæ; accuratiores fient. Atque istud prompte fiet, si prius tabulæ fue-
 “ rint constructæ ad singula scrupula dictæ parallaxis, singulosque senos vel
 “ quinos gradus cum argumenti æquationis sextæ pro æquatione sexta, tum dis-
 “ tantiae lunæ à sole pro septima.

“ A loco solis vero aufer medium motum nodi ascendentis lunæ æquatum ut

* This is the equation which Newton has here given with a *wrong sign*. The error is rectified in the second edition of the *Principia*, where Newton has entered into an investigation of the method by which it is deduced; and where the co-efficient is increased to $2'.25''$, which is the value adopted by Halley.

† In the second edition of the *Principia* the argument of this equation (which is here somewhat confused), as well as the co-efficient, is materially altered: and in the third edition, the equation is wholly omitted.

“ supra ; residuum erit nodi argumentum annuum ; unde ejus æquatio secunda
 “ computabitur modo sequenti : in figura præcedente referat ut prius T terram ;
 “ T S rectam jungentem terram et solem : referat porro T A C B lineam ductam
 “ ad locum nodi ascendentis lunæ ut supra æquatum, et S T A argumentum
 “ annum nodi. Capiatur T A ad A B ut 56 ad 3, sive $18\frac{2}{3}$ ad 1. Biseca B A in
 “ c, et centro c intervallo C A vel C B describe circulum A F B, fiatque angulus
 “ B C F æqualis duplo argumento nodi annuo ut supra invento ; eritque B T F
 “ angulus æquatio secunda nodi ascendentis, addenda in transitu nodi à solis
 “ quadrato ad syzygiam, subducenda in ejusdem transitu à syzygia ad quadra-
 “ turam. Atque sic habetur locus verus nodi orbitæ lunaris : unde ex tabulis
 “ methodo vulgari constructis supputabitur lunæ latitudo et reductio lunæ ab
 “ orbita sua ad eclipticam, positâ inclinatione orbis lunaris ad planum eclip-
 “ ticæ $4^{\text{gr}}. 59'. 35''$ cum nodi sunt in solis quadrato ; et $5^{\text{gr}}. 17'. 20''$ cum iidem
 “ in syzygiis versantur *. Ex modo inventis longitudine et latitudine et data
 “ obliquitate eclipticæ $23^{\text{gr}}. 29'$, lunæ ascensio recta et declinatio eruentur.

“ Lunæ in syzygiis mediocriter distantis à terra parallaxin horizontalem pono
 “ $57'. 30''$; motum horarium $33'. 32''. 32'''$; et diametrum apparentem $31'. 30''$:
 “ in quadraturis vero mediocriter à terra distantis parallaxin pono $56'. 40''$;
 “ motum horarium $32'. 12''. 2'''$; et diametrum apparentem $31'. 3''$. Lunæ in
 “ solis octante mediocriter distantis centrum distat à centro terræ quasi $60\frac{1}{2}$
 “ semidiametrorum terræ.

“ Solis parallaxin horizontalem pono $10''$: et $32'. 15''$ apparentem ejus dia-
 “ metrum in mediocri distantia à terra.

“ Telluris atmosphæra, refringendo et dissipando solis lumen, umbram pro-
 “ jicit, perinde ac si opaca foret, ad altitudinem minimùm 40 aut 50 milliarium
 “ geographicorum : (milliare geographicum appello partem sexagesimam gra-
 “ dūs magni circuli in telluris superficie :) umbra hæc in eclipsi lunari in
 “ lunam incidens telluris umbram auctiorem reddit. Et singulis milliaribus

* This paragraph, which contains the rule for the Reduction to the ecliptic, is left out in the second edition of the *Principia* ; but the detail is given in Prop. 35. I have not alluded to it in any of my remarks, as I have all along considered the moon as moving in her own orbit.

“atmosphæræ terrestris respondent singula scrupula secunda in lunæ disco.
 “Adeòque umbræ terrestris semidiameter in lunæ discum projecta augenda est
 “50 circiter secundis, aut (quod eodem recidit) in eclipsi lunari lunæ parallaxis
 “horizontalis augenda est in ratione circiter 70 ad 69.”

Si plura lunæ loca accuratissime observata (præsertim circa quadraturas) cum ejusdem locis ad eadem tempora ex supraposita theoria supputatis conferantur; patebit tandem num aliæ sint æquationes sensibiles, quibus theoria hæc est amplificanda*.

As it may tend to elucidate this subject still further, I shall here give, in the order laid down by Newton in the Rules above stated, the constants and arguments of the equations, for the moon's place in her orbit, expressed by the characters used by Delambre: where D denotes the angular distance of the moon from the sun, a the sun's anomaly, A the moon's anomaly, and Ω the longitude of the moon's node.

$$\begin{aligned}
 \text{Equat. 1} &= 11' 49'' \sin a && = \text{the Annual Equation} \\
 2 &= 3' 45'' \sin 2(D-A) \\
 3 &= 47'' \sin 2(\odot - \Omega) \\
 4 &= \text{Equation of the centre, including the Evection} \\
 5 &= 35' 15'' \sin 2D && = \text{the Variation} \\
 6 &= 2' 10'' \sin (2D + a - A) \\
 7 &= 2' 20'' \sin D.
 \end{aligned}$$

The maximum of the second equation, here assumed, is the mean of the two mentioned by Newton; as is likewise that of the fifth equation. They are each described as varying according to the mean anomaly of the sun: and the seventh is described as varying according to the position of the lunar apogee.

The four *new* equations are the 2nd, 3rd, 6th, and 7th. The second is called by Halley *æquatio prima semestris*: we have nothing equal to it in amount (depending on the same argument) in the tables of Mayer, Burgh,

* The last paragraph is Gregory's.

or Burckhardt*. The third is called by Halley *æquatio altera semestris*. The sixth is the same as Halley's fourth, the co-efficient of which he has increased to $2'.25''$, agreeably to Newton's subsequent amendment: and the seventh he has omitted altogether.

It will be evident, to the reader of the present day, that the whole expression furnishes but a rough numerical approximation to the formulæ now in use for denoting the true place of the moon in her orbit: and Flamsteed must have been a bad computer and a worse observer, not to have detected the errors which must necessarily have occurred on various occasions.

* No stress is laid upon this circumstance, since this equation, in the tables of Clairaut and Damoiseau, and in the formulæ of Plana, very nearly equals that of Newton. In fact, it is extremely difficult to exhibit a rigid comparison between the co-efficients of the lunar quantities adopted by different authors; arising from the various modes of developing the expressions that occur in the investigation.

ADDENDA ET CORRIGENDA.

(See the preceding list in page 667.)

Letter from Mr. Flamsteed to Dr. Smith. See page 723.

The Observatory, October 26, 1700.

KIND SIR,

I had yours of the 24th yesterday morning, and thank you kindly for the account you give me of the Arabic authors at Oxford; where I doubt not but there are ingenious young men enow who for encouragements would be at the pains to study that tongue, turn over those MSS. and translate such of them, as are useful: and I hope, rewards may be found for them. But of this I shall say more to you when we meet: therefore at present shall say nothing of them, nor would I have you mention my hopes, till I have discoursed [with] you. Those of them who have given us Catalogues of the fixed stars, I doubt not but had instruments, wherewith they took their meridional heights to determine their declinations: these are chiefly to be enquired after. As for those, who have only described their Astrolabes, they will be of little use to us.

Mr. Greaves, in your manuscript, derives the latitude of Oxford as Dr Bainbridge makes it, $51^{\circ} 46'$: but this Mr. Greaves deduces from the meridional heights of the solstitial sun; which being duely correct, allow it not more than $51^{\circ} 44\frac{1}{2}'$; which makes me think that Dr. Bainbridge found his from the same. I remember Mr. Halley, when at Oxford, used to make it $51^{\circ} 43'$: his instrument was small, but furnished with telescope sights, and seems to confirm my determination from Mr. Greaves's observation.

This winter (I hope with you) will compleat my Catalogue of the fixed stars, if God bless me with health; but 'tis a great work and will require a review to perfect it: and when 'tis done, a great deal remains behind to render my labors useful; of which those, who ask why I do not print, either do not think at all, or if they do, are perversely silent. To prevent you from continuing under misapprehensions of my endeavours you must allow me to trouble you with a short account of them, which I allow you to give to others, that do not understand my business, or are misinformed of it, to prevent further misapprehensions.

Tycho Brahé was born a hundred years before me; his observatory built 100 years before ours; he labored about 25 years on his Catalogue, had a large estate of his own, with about 3000 dollars per annum allowance from the King of Denmark, which in that cheap country and in those times was more than 3000 sterling here; he had 8 or 10 assistants. Yet with his large apparatus of instruments and help, he left us not more than 780 places of the fixed stars, rectified by himself and them, and about 40 places of each planet, deduced from his observations: of which those of the moon are of no great value; of all the other planets, allowable. When I sat down here I had only £100 per annum allowance from the office of the Ordnance, with a surly silly labourer paid by the office; till about 6 years ago, by the favour of Sir Thomas Littleton, Mr. Charleton and other officers, I got his pay allowed me and liberty to name the person myself. Sir Jonas Moore furnished me with two movements [clocks] and a large sextant, with which I began

my observations, Sept. 19, 1676. All the telescope glasses, tubes, and quadrants I procured at my own charge. I was promised a mural instrument, but a friend of ours contrived one so, that it was useless to me*; and an assistant, but he was never allowed: so that I was forced to take a young man to be my amanuensis and assistant at my own charge. I have ever since maintained one in that place now 25 years.

With the sextant, my own other instruments, and amanuensis, I continued my observations of the intermutual distances of the fixed stars from 1676 to September 1689. In the mean time I wanted observations of the stars' meridional zenith distances to connect them. I continued to get them with the sextant first, and after with a slight mural arc made at my own charge in 1683. But the first being an instrument not easy to fix, and the latter proving too slight, I durst not rely on the measures taken with either of them.

In 1688 my father died and left me some estate; and having then a servant fit for the business, and finding a large and weighty instrument necessary, I contrived and began a new mural arc. But my servant died before the ribs were joined together and I was forced to hire one that had served me 5 years before, and was both an excellent geometrician and mechanic, by whose help with 14 months continual labour and the expense of about £120 I finished it; and, finding it alone with the clock sufficient for my observations, have continued them with it from that time (October, 1689) to this present. By the help of observations taken with it, in the year 1694, I rectified the solar theory, and laid the foundation of the rectification of the fixed stars. Wanting help, and being loth to spend time in fruitless solicitations for it, I employed the year 1696 in computing tables, that might save me the help of a calculator, by abridging my work at least one half: which tables have been since of great use to me. All the year 1695 I had lost in sickness and furnishing Mr. I. N. [Isaac Newton] with the moon's places in order to restore her theory. Since the year 1696, I have rectified the places of the stars, not only in the ecliptical, but all the visible southern, and part of the northern, constellations: so that now I have by me about 1800 places of fixed stars, rectified in my Catalogue to the beginning of the year 1690. To which time I give not only their longitudes and latitudes (as in the old tables is usual) with their right ascensions and distances from the pole; but also the variations of each, whilst their longitudes alter an entire degree: for want of which, Tycho's tables are of little use. Whereas, with those, mine may be made to serve well and exactly for an age or two, past or to come; and by the help of some general tables, to be printed with them, for any term past or to come, supposing the recess of [the] equinoxes what you please. In the mean time I have calculated above 200 places of the moon, from observations taken at the full moons, quadratures and on her limits; as often as they have been observed betwixt 1689 and 1700. And the like for Saturn 30, Jupiter 40, and Mars 50 times in the said space. So that betwixt the years 1689 and 1700 I have done thrice as much as Tycho did with 10 assistants in more than double the time.

I have some of the northern constellations now under my hands, which will add above 600 stars to my Catalogue and yet will not complete it. The observations of the planets' places, taken with the sextant, cannot be examined, nor the places derived from them, till the Catalogue be finished. They are very numerous, and ought to be derived before anything be published; otherwise unexpert persons will deduce them erroneously, and lay the blame on him that published them. They will require very skilful persons, and ready at numbers to manage them. I have caused the calculation of every star's place, that has past under my hands, and each place of the

* The "friend," here alluded to, is evidently Mr. Hooke: see page 46. F. B.

moon, to be repeated, to avoid errors: the same course must be observed here; so that two persons do really but one man's work. The calculations of the moon's place must be done with great care and require persons of extraordinary skill to manage them, by reason of the perplexed consideration of parallaxes. And a many of those of the moon's places I have observed since 1689, but omitted for want of help, must be taken into consideration, and the places derived from them, before they be published. So you see that 'tis purely want of help that causes me not to publish what I have done: and that when I shall be so happy as to publish it, the world (that part of it I mean, that is unprejudiced) will be satisfied that I have not mispent my time.

Some persons say, "but I might publish something in the mean time, to stop the mouths of clamorous persons." Should I attempt any such thing, the main work in the mean time must stand, which its not worth my while to defer, in hope to silence clamorous persons. They know nothing of my work, but as 'tis represented to them; and those who set them on, know not much. A physician or a botanist would not heed me much if I should urge him to publish a book of plants: nor ought I to heed those, who urge me about what they have much less skill in, than I have in botanicks or physick.

But they want my Catalogue and lunar observations, and desire no more. Softly pray. 'Tis surely requisite [that] the observations of the fixed stars, on which the Catalogue is grounded, should be published first: next the Catalogue of the fixed stars; and lastly the planets' places derived from both. To publish them in any other order, would be preposterous and mean. I must consult the honour of the King and nation and not the particular satisfaction of any private person, that clamours and makes a noise, because he wants preferment, or thinks he may command my pains, because he has great friends in eminent posts. I know what I have to do, and will not receive directions from them, who presume to talk much amongst the ignorant of what they know but little themselves. I am above £1000 out of pocket in this employ already: and the charge of my family and assistance is every year about £100 more than my salary and benefice together make me. I think I needed not to have mentioned the last, but that some people esteem it much more than it is. I think I have no reason to spend more on a work, that lies me in so much already, and which perhaps some persons would only carp at, after they have called so earnestly for it. Briefly Sir, I am ready to put the observations into the press, as soon as they, that are concerned, shall afford me assistants to copy them and finish the calculations. But if none be afforded, both they and I must sit down contented, till I can finish them with such hands as I have; when I doubt not, but to publish them, as they ought to be, handsomely and in good order; and to satisfy the world, whilst I have been barbarously traduced by base and silly people, that I have spent my time much better than I should have done if to satisfy them I had published anything sooner and imperfect. Pray be no niggard in reporting what I have told you for the satisfaction of others, though it be wrote chiefly for yours, by Sir,

Your friend and obliged servant,

JOHN FLAMSTEED, M.R.

I am in haste—pardon the slips of my pen.—J. F.

[From the *original* in the Bodleian library, Oxford.]

Letter from Dr. Smith to Mr. Flamsteed. See page 723.

London, October 31, 1700.

SIR,

I have read your letter of the 26th with extreme great pleasure, for which I give you my hearty thanks. The reasons, which you there allege, for your not publishing your catalogue of the fixed stars, your observations of the planets' places, and especially those of the moon, seem to me very just and highly satisfactory; and which I shall mention upon occasion, and upon any accidental discourse reflecting upon your delay; and shall be enabled thereby to defend you from any attack, which may be made [against] you on that side. In the mean while my advice is, that you contract into a close and well connected sum the substance of your large apologetic letter to me; and represent your case nakedly, clearly, and without any flourish, or without any kind of resentment (as you are a Philosopher and a Mathematician, and above all, as you are a Clergyman), and send copies to the great men, who are so solicitous for the great work you have been employed upon for so many years; and who are so dissatisfied, that you have printed nothing of it all this while, (they forget your learned letter to Dr. Wallis and the admirable proofs there of the parallax of the annual orb of the earth) in order to convince them that it has gone on slowly for want of public assistance, and that you cannot print your catalogue and lunar observations by themselves till the whole be finished, without great prejudice and inconvenience to the main design, the several parts of which mutually illustrating and supporting one another. This, I think, should be done out of hand; the entire consideration of which I leave to your wiser and better thoughts. I hope, that you will review your sermon preached at Greenwich against Atheism; which I wish too that you would enlarge into a little treatise. Such a discourse, as to the newness of the method, and unusualness of the arguments, being likely to do most good upon the wits of the age, who with equal ignorance, and impudence set up for Deists and Atheists. If you think fit to trust me with these papers, I will take effectual care to have them elegantly and correctly published. I am, Sir,

Your most faithful friend and servant,

T. S.

[From the *original* in the Bodleian library, Oxford.]

N.B. Flamsteed's answer to this letter has already been printed in page 669.

Letter from Mr. Flamsteed to the Rev. S. Thornton.

Observatory, February 18, 1702-3.

REV. SIR,

When you were last here I took occasion to show you some papers, whereby it was evident that the theory of the [magnetic] variations published by Mr. Halley, now near 20 years ago, on which he pretends to ground his maps of them, was the foundation of Mr. Perkins', whose papers he bought at a small rate: which because I would not disingenuously and dishonestly conceal, he has made it his business to ridicule me behind my back, both in the [rooms] of the R. S. [Royal Society], and in all companies where he resorts*. Living in London (where I am but seldom)

* This is a repetition of the charge of plagiarism which is alluded to in Flamsteed's letter to Perkins in page 193. From the tenor of that letter it would appear that the quarrel between Flamsteed and Halley commenced about the

and frequenting the company of lewd young gentlemen, he has had an opportunity to repeat his calumnies, and spread them even into your neighbourhood, and to represent me as a spiteful envious person. I saw, by your smiles when here, on several occasions, that you were possessed with his representations; and therefore, to undeceive you, desired your company at Garraway's, by a letter that has occasioned yours; wherewith I am very well satisfied, and hope that hereafter when you hear me misrepresented, you will do me justice; especially amongst your brethren of the clergy, and particularly with Mr. Petit, who I hear has made bolder with me than became him. I have reprov'd him for it; and am not the less the friend of one who has rather erred through ignorance than malice.

I thought you would soon have enough of Dr. Gregory's book. Mr. Halley says he is a churchman too. They are confederates; but I believe have no confidence in one another. Mr. Halley saw his book before it was printed: I was not vouchsafed the sight of it; the reason is plain to you. But I feared the letter I wrote to Mr. Caswell (of which I showed you a copy) makes you have a less opinion of it, than you would otherwise*. You thought to have found Mr. Newton's Principles made easier by him: but except you read Mr. Newton's preliminaries, you would not understand Dr. Gregory. When you have got him, he misleads you into a perplexed theory of the moon, encumbered with menstrual inequalities, which Mr. Newton justly makes annual, as they are in the Horroxian theory: to which, by the help of what has been imparted to him from the observations, he has given some new corrections and additions, which will make it agree better with the heavens than the old tables did†. If you compare Dr. Gregory's $\mathcal{D}$ with Mr. Newton's, you will find little resemblance, though they ought to be the same: as Kepler's theory, published in the Rudolphine Tables is with that derived from him by Mr. Horrox; which much resembles the old one of Hipparchus, published by Ptolemy, employed by Alfonsus and Copernicus, [and] improved by Tycho and Kepler; which is therefore now almost 2000 years old. You tell me you have taken some propositions on trust, from the Doctor. I believe you need not suspect his sincerity or abilities in anything of geometry, though his astronomy be poor. He is fitter for the other chair.

I thank you for your wish of health and happiness to all the family, and shall take it kindly if you will always call here as you go to London. In the meantime, as occasion serves, let me hear of your health by a letter, to, Sir, your sincere friend and servant,

JOHN FLAMSTEED.

P.S. I am very busy in fitting up my large catalogue of fixed stars. God has blest my labour; I praise him for it: and I hope the cold, I have been troubled with, leaves me so that I can better follow than formerly. I hope to have it in order before you come here.

For the Rev. Mr. Steven Thornton,

Rector of Ludsdon, Kent.

[Copied from the *original* letter in the possession of the Hon. and Rev. G. N. Grenville.]

year 1682, soon after the return of the latter from the continent. The cause of Flamsteed's animosity is probably correctly detailed in the present letter: and corresponds with the opinion which I have hazarded in the note in the Preface, page xxxi. F. B.

* See the two letters of Flamsteed to Caswell, on this subject, in pages 203 and 205. F. B.

† This was written prior to the examination of the new equations introduced by Newton: see Flamsteed's third letter to Caswell, in page 213. F. B.

Note to page vii. of the Preface.

The publication of the evidence which I have adduced in Flamsteed's vindication has had the effect of removing from the mind of Miss Hutton (as, I trust, it has from that of the whole world) every unfavorable impression that might have been caused by the unfounded report so incautiously circulated by her father. As Miss Hutton's recent letter to me on this subject does equal credit to her head and to her heart, and evinces her love of truth and justice as well as her filial affection, I shall not offer any apology for here inserting such extracts from it as relate to the case in question.

"You have (she says) fully cleared Flamsteed of the crime alleged against him by my father, on the authority of Mr. Webb: and, as I cannot doubt the integrity of the one, or the accuracy of the other, I suppose that Mr. Webb must have heard the circumstance privately from some person whom he thought trustworthy, and have related it as a fact; and that my father published what he could not but believe. I think the affair could not attach to the kinsman, because Mr. Webb lived too near the time to mistake him for the astronomer; even if he were not acquainted with him (the astronomer), as he said and as I believe he was. And if the kinsman had been guilty, his pardon would have been found in the records.

"But may I own that I think you have borne rather hard upon my father. He was a *lover*, not a *collector*, of anecdotes. When he had heard them, they rested, unconsciously to himself, in his mind till occasion called them forth; when they rose to his recollection precisely as he had heard them. His memory was so extraordinary that, at seventy-five years of age, he wrote his past life, as it appeared in public, without having one written line to refer to.

"My father was not ashamed of his *low origin*, nor am I: but can this be considered as a proof of his incorrectness? Good understanding and good memory know no distinctions of rank: and these qualities have, for successive generations, been hereditary in his family. Nor could the meanness of my father's condition affect Mr. Webb, who had seen better days and more enlightened company. I was shocked when I came to the *wild pranks* of my father. He had no wild pranks. The affair alluded to was a melancholy one:—so melancholy that I, as a daughter, never could read it without tears.

"You finally acquit my father of *misrepresentation either wilful or accidental*. My dear sir, why did you add '*since it is very probable that he heard (or believed that he heard) the anecdote from Mr. Webb.*' It is not only probable, it is unquestionable. But I agree with you in charging my father with a *want of prudence and caution* in publishing such an allegation on such a slight authority."

One part, and one part only, of this letter requires any comment from me. Miss Hutton assumes that my reference to the "low origin" of her father was intended as a reproach, whereas my allusion to it was merely to show the improbability that a person in his then state of life could have the means of judging correctly of the reports which he may have heard respecting a person moving in so different a sphere from himself as Flamsteed did. So far from being a reproach, I consider Mr. Hutton's emancipation of himself, by his own exertions and abilities, from the thralldom of poverty and low company, as one of the brightest and most honorable points in his history; and would, even in this enlightened age, mark him as an extraordinary character.

Note to page 72.

In the second note in page 72, I have mentioned that Mrs. Catherine Barton, the niece of Newton, was the *widow* of Col. Barton. This was stated on the authority of all the printed accounts that I have yet seen. But I have since found, from the inspection of some original documents, collected with great care by Professor Rigaud, that she was not the *widow*, but the *sister*, of Col. Barton. For, Newton's half sister, *Hannah* Smith (not *Mary*, as stated by some writers) married Mr. Robert Barton; and Col. Barton and Mrs. Barton were the issue of that marriage: the title of *Mrs.* being, in that day, given to young *unmarried* (as well as married) ladies, when they arrived at the age of puberty. I have not been able to ascertain the date of the birth of Col. Barton; but he died in the expedition under Gen. Hill, against Quebec, in 1711. Mrs. Barton, from the best information I have been able to obtain, was born about the year 1680; and, at the time that Newton was made Warden of the Mint, must have been about 16 years of age. So that the statement made by her cousin, the Rev. Benj. Smith (in Whitaker's *History of Craven*, page 462) that she was not *then* born, is singularly erroneous: more especially as it is well known that Lord Halifax, in the *first* codicil to his will, signed April 12, 1706, makes the following bequest. "I give and bequeath to Mrs. Catherine Barton all the jewels I have at the time of my death, and likewise £3000 as a small token of the great love and affection I have *long* had for her." And, in a *second* codicil signed February 1, 1712, after revoking the preceding bequest, and leaving her other property to a much larger amount, he adds "these gifts and legacies I leave to her as a token of the sincere love, affection, and esteem, I have long had for her person, and as a small recompence for the pleasure and happiness I have had in her conversation." There are indeed several other mistakes in Mr. Smith's account of the genealogy of his family, to which however it is not necessary here to refer more minutely. But the *burning* of Newton's letters must always be regretted, notwithstanding they are said to have contained "some vulgar phraseology."

Page Line

xiii 3 For "Edward Giles" read "Edmund Giles."

lxvi 8 Since the printing of the first part of this work, the present Astronomer Royal has discovered another MS. of Flamsteed's at the Royal Observatory, which had escaped my research; and which he has been kind enough to forward to me for my inspection. It is a book of reductions similar in arrangement and design, to the books marked A, B, C, in Vol. 26; and has an index at the end: it is now marked with the letter F, and will be found in the portfolio, Vol. 26. It has furnished me with the means of making two further corrections of the British Catalogue, which will be presently mentioned.

28 Note The *original* MS. of the first communication of Flamsteed to the Royal Society is in the possession of the Earl of Macclesfield. This document I had an opportunity of perusing during the present year, when I was permitted by his Lordship to inspect the valuable collection of original letters, at Shirburn Castle, from various eminent persons who flourished about that period: which document, together with many of the letters above alluded to, will probably, through the kindness of his Lordship, and his well known zeal for the promotion of every branch of science and literature, be soon made public.

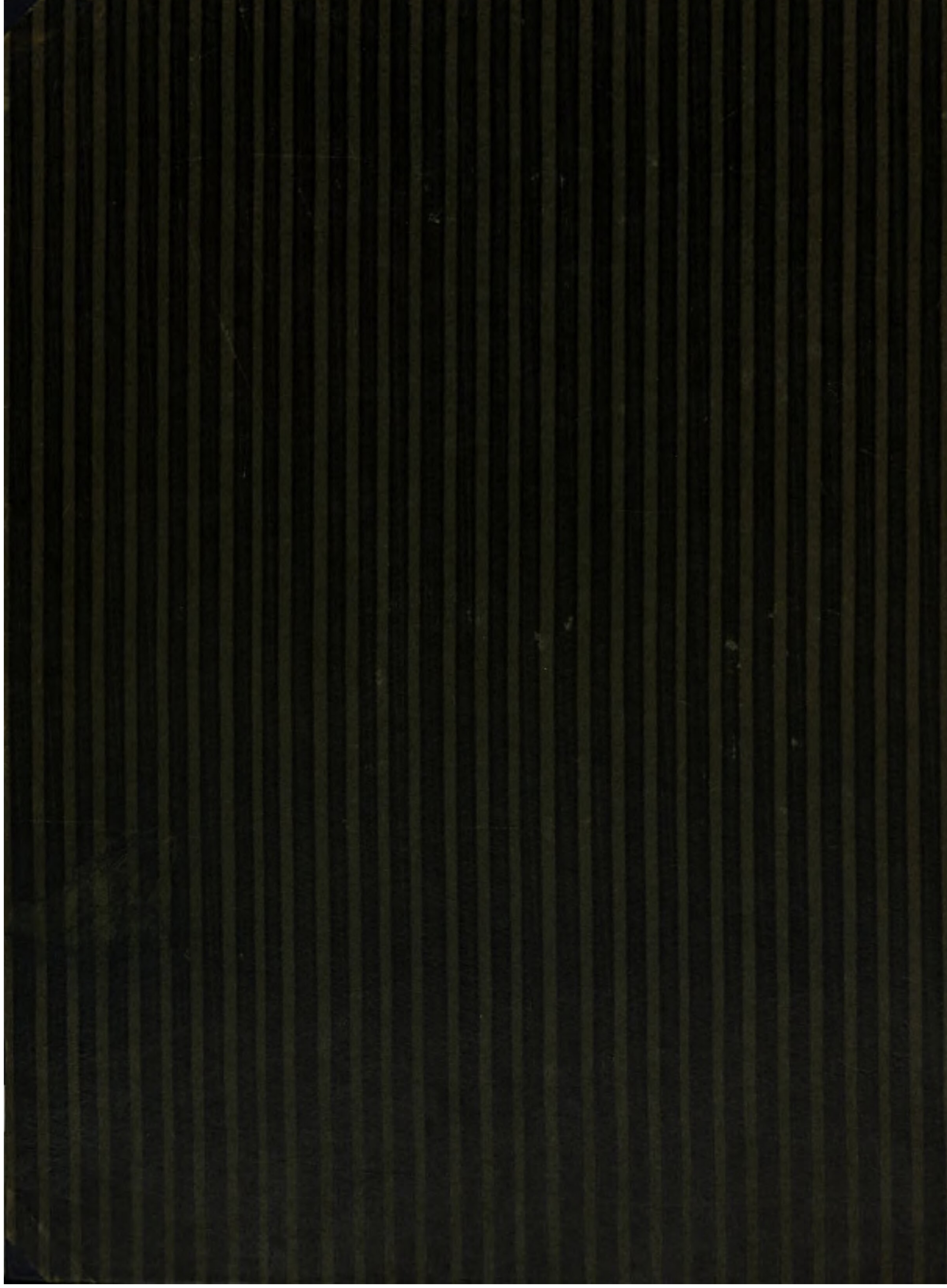
36 24 For "36" read "37."

151 3 from the bottom. For "worse" read "more."

165 6 from the bottom. For "Sir Isaac Newton" read "Mr. Isaac Newton."

- | Page | Line | |
|------|------|--|
| 199 | 9 | For "that" read "as." |
| 211 | 8 | For "5° 0', 5° 12', and 5° 18'," read "5° 0', 5° 6', 5° 12', and 5° 18'." |
| 236 | 13 | For "the order" read "your order." |
| 245 | 3 | For "any" read "my." |
| 246 | 8 | from the bottom. For "gave" read "give." |
| 247 | 12 | from the bottom. For "that you" read "that is, you." |
| 248 | 5 | from the bottom. For "the D's tables" read "the lunar tables." |
| 249 | 9 | For "between" read "betwixt." |
| 250 | 2 | from the bottom. For "determined" read "examined." |
| 268 | 1 | For "last proved" read "last has proved." |
| 299 | 21 | The word "deal" should have been placed within brackets. |
| 309 | 4 | from the bottom. Insert the word "sixth" after the word "his." |
| 310 | 11 | For "Durham" read "Derham." |
| 313 | 20 | For "April 23" read "April 28." |
| 315 | 4 | For "it to" read "it [to]." |
| 321 | 14 | The word "of" should have been placed within brackets. |
| 327 | last | For "2' 26'" read "2° 26'." |
| 328 | 4 | For "is as good" read "is [as] good." |
| 422 | | No. 410. Dele ϵ , and cancel the reference to the note. |
| 525 | 26 | Cancel the note to No. 410. |
| 545 | | In addition to the note to No. 913, I would remark that in the newly-discovered MS. Vol. 26 F, page 9 (above mentioned), the observation of Feb. 19, 1696, from which the position of 21 <i>Geminorum</i> is deduced, is marked as doubtful; and that it is circumflexed with two reduced observations of 20 <i>Geminorum</i> , as if Flamsteed had considered them all as one and the same star. |
| 554 | 13 | In addition to the note to No. 1098, I would remark that, by means of the newly discovered MS. mentioned in the preceding note, I have ascertained how the erroneous insertion of this star has arisen. It is in fact the same star as No. 1099 (immediately following) which was observed on March 4, 1693, at 7 ^h .54 ^m .58 ^s : for I find, in page 44 of the above MS. that Flamsteed, in there reducing that observation, has taken the uncorrected <i>zenith</i> distance (viz. 76°.40'.20") for the correct <i>polar</i> distance, and has inserted it as such in his catalogue; thus making two stars out of one observation. When this error is corrected it will be seen that No. 1098, 12 <i>Canis Minoris</i> , should be erased from the catalogue. For the same reason also it should be erased from Table D in page 646, and inserted in Table B in page 645. |
| 653 | 2 | For "answer" read "account." |
| 665 | 21 | For "19,44" read "19,56." |
| 669 | 20 | Fill up the blank with the word "knows," which had accidentally dropped out of the printer's form, during the working of the press. |
| 671 | 20 | In addition to the note to page 212, there given, I will here subjoin the passage from Flamsteed's letter of August 5, 1703, referring to the subject there alluded to. "You have judged right (says he) who I meant by Mr. Raymer: 'tis the name of the person who made it his business to depreciate Tycho Brahé, after he had been courteously entertained by him, and assured him of his inviolable friendship." The character of Nicolaus Raymarus (<i>Ursus Dithmarsus</i>) is described in no very flattering terms by Gassendi, in his Life of Tycho Brahé: and Flamsteed's animosity towards Halley could not be more acrimoniously expressed than by giving him that appellation; however unjust we, of the present day, may consider the imputation. |





Baily, Francis

Supplement to the account of the Rev. John Flamsteed

London 1837

4 Biogr. 101 n

urn:nbn:de:bvb:12-bsb10049771-0